THURSDAY, AUGUST 4, 1881

FOSSIL CRINOIDS

Mémoires de la Société Paléontologique Suisse. Monographie des Crinoïdes Fossiles de la Suisse. Par P. de Loriol. (Genève: Imprimerie Charles Schuchardt, 1877–1879.)

Iconographia Crinoideorum in Stratis Sueciæ Siluricis Fossilium. Auctore N. P. Angelin, Opus postumum edendum curavit Regia Academia Scientiarum Suecica. Cum Tabulis XXIX. (Holmiæ: Samson et Wallin, 1878.)

PROF. P. DE LORIOL of Geneva, who is so well known for his researches on the fossil sea-urchins, has been occupying himself for some time past with the study of the fossil Crinoids. A handsome volume, consisting of 300 pages of text and twenty-one somewhat crowded quarto plates, contains the results of his work on those discovered in the stratified rocks of Switzerland. It originally appeared in three parts, which formed portions of the volumes issued by the Palæontological Society of Switzerland for the years 1877-79.

The total number of species described by Prof. de Loriol amounts to 125, of which thirty-nine are new to science. The series commences with the well-known "Lily-Encrinite" from the Muschelkalk, and ends with a species of D'Orbigny's doubtful genus "*Conocrinus*" from the Nummulitic Eocene of Wesen. Palæozoic Crinoids are, of course, conspicuous by their absence; so that Prof. de Loriol was not hampered by having to deal with any obsolete system of classification. For the primary divisions of the class he adopts Dujardin's modification of Pictet's system. This throws such very diverse forms as *Encrinus*, *Apiocrinus*, and *Pentacrinus* into one family, the *Pycnocrinides*, which is especially characterised by the thickness of the plates of the calyx.

Each of these genera, however, is best regarded as the type of a separate family. In fact, Pictet's "family" of *Pycnocrinides* includes nearly all the non-palæozoic Crinoids or Neocrinoidea except the *Comatulæ*, and is far more comprehensive than an ordinary zoological "family."

Encrinus and *Apiocrinus* are fairly well represented in the Jurassic rocks of Switzerland. Two species of the former genus are described by Prof. de Loriol, one of which is new; and there are four species of *Apiocrinus*, one of which is new, though founded only on the characters of the stem. *Millericrinus* and *Pentacrinus*, however, are considerably more abundant. Thirty-three species of the former are described, two being Liassic and three Cretaceous; while there are no less than forty-three *Pentacrinus* species, six of which are Cretaceous, and one from the Infra-Lias (Rhætic).

Most of the species are necessarily founded only on the characters of isolated joints and fragments of stems, and are therefore only of provisional value; for two or more joints, the markings on which differ considerably, may really belong to different parts of the same stem. Nevertheless, after making allowance for these possibilities, Prof. de Loriol finds a considerable number of different types of stem which are confined to particular horizons. They thus acquire some stratigraphical value, and it is convenient to name them, but the names can only acquire a permanent value (or otherwise) when we are acquainted with the calices associated with the stem-joints in question. This is, unfortunately, but far too rarely possible.

The genus "*Pentacrinus*" is a large one, and it is almost necessary to separate off some of the best marked varieties as distinct generic types, just as has been done with *Apiocrinus*. Prof. de Loriol has attempted this subdivision in two cases, in one of which he seems to us to be fully justified, though we cannot say the same for the other. He attempts to re-establish the genus *Cainocrinus* of Edward Forbes, to include those species of *Pentacrinus* in which the basals form a complete ring and cut off the radials entirely from the top stem-joint. The characters of the stem and of the faces of its component joints are identical with those of the ordinary *Pentacrinus* type; and there is so much variation in the development of the basals among the different *Pentacrinus* species, both recent and fossil, that it is hardly worth while to separate off one of the extreme terms of the series as a distinct genus. Besides the fossil species mentioned by Prof. de Loriol *Cainocrinus* would include the recent *Pentacrinus Mülleri*, Oersted, from the Caribbean Sea, *P. Wyville-Thomsoni* from the North Atlantic, and *P. Maclearanus* of the *Challenger* dredgings.

The genus *Balanocrinus* was established by the late Prof. Louis Agassiz for a crinoidal fragment that he believed to be a calyx with an attached stem-joint; and he described the terminal face of the latter as resembling those of the stem-joints of *Pentacrinus subteres*. Prof. de Loriol, however, finds this fragment to be merely an abnormally swollen piece of stem, with the borings of some parasitic mollusc. But the stem-joints of *P. subteres* have rather different terminal faces from those of the ordinary *Pentacrinus* species; and Prof. de Loriol therefore proposes to retain the name *Balanocrinus* for this and similar forms, in which only the rim of each joint-face is crenulated, and not the central ends of its petaloid divisions as in the ordinary *Pentacrinidæ*. No calyx has ever been found associated with stem-joints of this nature except perhaps that of *P. Fisheri*. This name was given by Edward Forbes to a specimen from the Oxford clay of Weymouth that was described by Baily, who did not, however, say much about the stem-joints. Prof. de Loriol directs the attention of English palæontologists to this subject, in the hope of finding out whether Baily's species is a *Balanocrinus*. If it be so, the original specimen would acquire additional value from its being the only one with the calyx preserved.

The well-known genus *Eugeniacrinus*, which is made the type of a new family by Prof. de Loriol, is represented in the Swiss rocks by nine species, ranging from the "Oxfordien" to the "Néocomien." The curious form *Phyllocrinus* with its deeply incised radials was described by d'Orbigny as a Neocomian Blastoid allied to *Pentremites*; but it has become less interesting since Prof. Zittel showed it to be a near ally of *Eugeniacrinus*. It is represented in Switzerland by nine well-marked species, which range from the Lower Oolites to the Neocomian deposits.

Comatulæ are also abundant in the Swiss rocks, twelve species being described by Prof. de Loriol, eleven of

which are new. These are equally distributed through the Jurassic and Cretaceous series; but there are none as old as our own *Actinometra Cheltonensis* from the Inferior Oolite of Gloucestershire, nor as young as various species from the Margate chalk. One of the Neocomian species belongs to the sub-genus *Ophiocrinus* of Semper, which is characterised by the presence of five undivided rays. There are only three recent species referable to this type, all of them inhabiting different portions of the Pacific Ocean. With the *Comatulæ* must be included two species of the curious genus *Thiollierierinus*, recently mentioned in these columns (vol. xxiii. p. 377) as being a permanent larval form.

Prof. de Loriol's Monograph with its abundant illustrations forms an excellent supplement to the fourth volume of Quenstedt's wonderful "Petrefactenkunde Deutschlands," which deals with the *Encriniden*. Taken together, the two works give us a very complete account of the Mezozoic Crinoids of Central Europe. We understand that Prof. de Loriol is now working out the French Crinoids in the same way as he has treated the Swiss ones, and we hope that he will be enabled to complete this somewhat extensive task with an equally satisfactory result. This will render a similar work on the British Crinoids more than ever necessary, and we trust that it may be accomplished within a reasonable time.

The second book mentioned at the head of this article is the late Prof. Angelin's "Iconographia of the Silurian Crinoids of Sweden." It has been published as a posthumous work by the Swedish Academy, and is unquestionably the finest work on Crinoids that has ever appeared. It consists of twenty-nine beautifully-printed folio plates, which illustrate the marvellous wealth of Crinoids and Cystids in the Silurian rocks of Sweden. Some of the figures, such as those of *Crotalocrinus*, are excessively intricate, and they are all admirably clear and well-arranged. The lamented death of the eminent Swedish palæontologist has unfortunately prevented these figures from being as useful to his successors as they would have been, had he lived to describe them. They have been edited by two of his colleagues, Professors Lovén and Lindström, who have classified the genera and species according to the system which they found sketched out in Prof. Angelin's notes and manuscripts. Unfortunately, however, the classification is an entirely unnatural one, depending upon the number of basal plates in the calyx. Wachsmuth, the chief authority in America on the Palæocrinoids, has already pointed out that while it brings together very distinct types such as *Rhodocrinus* and *Poteriocrinus*, genera which are very intimately related, such as *Platycrinus* and *Dichocrinus*, are widely separated. Among the true Crinoids forty genera are figured, comprising 176 species, many of which are new. They are arranged into twenty-three families, but as these are not defined we are unable to learn the principles upon which they were established.

There are also figures of twenty-three Cystidean species, arranged into nine genera, including one new one, which fall into three sections, the *Apora*, *Gemellipora*, and *Rhombifera*. So far as can be judged from the species referred to each section, Angelin's classification is something more than an introduction of new names for the three divisions of the group which were sketched out by Müller. Neither of the three genera included in the *Apora*, Angelin, are ordinarily referred to the *Aporitidæ;* but *Echinosphærites aurantium* and *Caryocystites*, von Buch, were placed by Müller among the *Rhombiferi* or "Cystideen mit Porenrauten"; while the third genus, *Megacystites*, Hall, is ordinarily referred to the *Diploporitidæ*, which is a parallel group to the *Gemellipora*, Angelin.

As in the case of the true Crinoids, we are unable to learn the principle of Angelin's classification of the Cystidea. It is not likely therefore to be adopted, at any rate for the present. Possibly, however, it may stand the test of future discoveries better than the Müllerian system, though we do not think this contingency a very probable one.

In spite of the inconsistencies which we have mentioned, the "Iconographia" must be indispensable to every student of the Palæocrinoidea. A glance through its pages makes one long to see some really good illustrations of our British species. There are many specimens of the utmost beauty and novelty, both in our public museums and in private collections, which we hope will some day be properly described in a "Monograph of the Fossil Crinoids of the British Isles."

OUR BOOK SHELF

The Countries of the World. By Robert Brown. Vol. vi. (London: Cassell and Co.)

We are surprised that, after so many volumes of this work have been devoted to the description of America and Asia, the whole of Europe and of Africa are disposed of in a single volume, a considerable part of it being devoted, moreover, to the Turkish Empire. This last is allotted 58 pages, whilst the whole of Europe is dealt with in 104 pages, and the whole of Africa in other 104 pages. Moreover, why should Turkey have the favour of receiving thrice as much space as Russia, which is actually dismissed in only eight pages, whilst France, Germany, Italy, and Spain have only four pages each. Does the Russian Empire, or Spain, with their variety of climate, of soil, and of population, afford less interest for the general reader than Asiatic Turkey, and Italy less than Senegambia or Liberia?

It is obvious that such a distribution of space must affect the entire value of the work. Certainly when reading Mr. Brown's book we have admired in many instances the talent with which he succeeds in condensing in to very few pages a good description of a country; but the book being intended to afford more interest to the general reader than a simple text-book of geography, the author has been compelled to enter into generalisations which cannot but give a false idea of the subject. Is it possible that the reader can have a true conception of the climate of France when he learns from Mr. R. Brown's book that "the climate is one of the finest in Europe—mild, equable, and healthy, in spite of the hot winds from Africa, which sometimes impinge on the southern districts, and the chilly 'mistral' which sweeps down from the Alps in the north"? Or, what an idea will be impressed upon his mind of Paris, when he learns only that "in Paris centres the most polished society of the world. From Paris are sent forth *the books, the bonnets, the pictures*, and possibly even the vices which are so largely aped by the rest of the civilised world. It is the city of pleasure. But, contrary to the general impression, the morals of Paris, if not high, are not superlatively low; for though these are depraved enough, they are infinitely superior in many respects to those of Vienna, Naples, Bucharest, and even Berlin, which is more cir-

cumspect and prudish." All this is quite right, but is it a description worthy of the great capital of the Continent? The same might be said of all the other countries touched by Mr. R. Brown in this volume. All that he says is quite correct, and we do not find such blunders as are too often found in geographical works. But the necessity of giving the reader a generalisation for the purpose of rendering the book more interesting often leads the author to make such generalisations as give to the reader a most untrue conception of the subject. We must regret that Mr. Brown has been compelled to condense his work in this way, and thus seriously diminish the value of what promised to be a useful and trustworthy compilation.

Phonetik. Zur vergleichenden Physiologie der Stimme und Sprache. By Dr. F. Techmer. (Leipzig: Engelmann, 1880.)

THE excellent work on the Physiology of Language published by Dr. Techmer under the above title forms the first volume of an Introduction to the Science of Language, the rest of which is hereafter to appear. We have little hesitation in saying that it is the best *résumé* that exists at present of what is known about the nature and formation of the sounds we utter.

Dr. Techmer has been well prepared for the task he has undertaken. In the first instance a student of natural science, he next devoted himself to the acquisition of modern European languages, then of languages so remote from ours as Chinese and Sanskrit, and finally to the study of comparative philology. Naturally, however, his earlier studies had inclined him rather to the investigation of the material of speech than to the antiquarian researches of the Indo-Germanists or the psychological inquiries of the school of Steinthal. He brought to the investigation a well-trained mind, an intimate acquaintance with physics, acoustics, and physiology, a wide range of reading, and keen observation. What he has to say, therefore, is well worthy of attention.

The ground he covers is so extensive that in order to bring his work within manageable compass he can do little more than indicate the chief facts, methods of investigation and results which have been arrived at by previous phoneticians, along with copious references and notes. These will enable the reader to follow each particular point into special detail, if he so wish. At the same time Dr. Techmer has not been content with being merely a passive reproducer of the opinions of others. He has carefully tested them wherever it has been possible, and made independent experiments of his own, the results of which he lays before us. Hence his judgments and criticisms are always of value, while the numerous and carefully-drawn illustrations and diagrams which accompany his work leave little to be desired.

He has done well in not forgetting the comparative method in his treatment of phonetics. Properly to understand the physiology of human speech it is necessary to compare our vocal organs with those of reptiles, mammals, and more especially birds. Jäger has already been struck by the curious relationship that seems to exist between the power of speech and walking on two feet, and has endeavoured to explain it, though not very successfully.

Perhaps the fact that is most brought home to our minds by a study of Dr. Techmer's book is the uncertainty and obscurity that still hang over a large part of phonetics. Experts still differ radically on some of the most fundamental details of the science. This is more especially the case with that side of the science which has to do with acoustics; on the physiological side it lends itself more readily to observation and experiment, and the physiological conditions requisite for the production of particular sounds are consequently much better known. Hence it is that the nature of the consonants is far more accurately determined than that of the vowels, and that it will be long before all the difficulties connected with the formation of the latter are satisfactorily removed. The best means of overcoming them will be a succession of works like this of Dr. Techmer's, at once clear, precise, and thorough.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Medusæ

IN Mr. H. N. Moseley's "Notes by a Naturalist," on the *Challenger*, p. 404, a curious habit of Medusæ in the Island of Santa Cruz Major, is mentioned, viz. their lying on the tops of their umbrellas, its tentacles directed upwards. I think your readers may be interested to learn that I have frequently noticed Medusæ in a similar position in the West Indies. A few years ago I was quartered for some time at Port Royal, Jamaica, and in the channels between the mangroves I observed what I at first thought were Actineæ of large size on the muddy bottom, in about eight feet of water. They were very numerous. I stirred one up with the boat-hook, and was surprised to find it was a Medusa turned upside down. On being disturbed, it lazily contracted its umbrella in the usual manner and settled down again in the mud as before. The species was about a foot in diameter of umbrella, and dirty white in colour. I never saw them swimming in the mangrove creeks, though I was frequently out in a boat, and they were at all times common on the bottom, lying as described. Some time afterwards I saw what seemed to be the same species at St. George's Bay, a small island about ten miles from Belize, Honduras. It was lying in the same position on the mud amongst the mangroves, in about four feet of water. I poked several up with a stick, and they slowly swam for a short distance, and again settled down on their umbrellas. I believe it to be really the habit of the species to lie on its back, as it were, and it is interesting to find another kind in the east acting similarly. Mangrove swamps are extensive in the vicinity of Singapore, but I have not noticed any Medusæ here in that position, possibly because there is a considerable tide which leaves the mud bare at low water.

I think I have seen the habit noticed in some book, but cannot recollect where. H. ARCHER

Fort Canning, Singapore, June 28

Two Kinds of Stamens with Different Functions in the same Flower

THE following extract from a letter lately received from my brother Fritz Müller (of Blumenau, Prov. St. Catharina, Brazil) contains so new and curious an observation that it will probably interest the botanical readers of this journal.

"A species of Heeria (Melastomaceæ), which is not indigenous here, begins in my garden now to open its beautiful red flowers,

Flower of Heeria spec., longitudinally dissected. *s*, sepals; *p*, petals; a^1, one of the conspicuous yellow anthers which attract the insects; a^2 one of the inconspicuous red anthers, which powder the insects with pollen; *c*, connective of this anther; *f*, fork of this connective; *st*, stigma.

remarkable for having two kinds of differently coloured anthers. The four petals spread in a perpendicular plane; the yellow anthers (a^1) of the four shorter filaments, closely pressed together, project from the middle of the flower; their bright yellow strikingly contrasts with the violet-shining light red of the corolla; the longer anthers (a^2) are red, like the filaments, and the very long connective (c), which is lengthened beyond the point of insertion

into a fork (*f*), with two yellowish points; these points stand close beneath the yellow anthers, whilst the apical apertures of the red anthers (a^2) are placed far below them near the stigma; also the style and the stigma (*st*) are coloured so very like the corolla, that from some distance neither they nor the longer stamens can be seen at all. Any large bee (like Xylocopa, Centris, or Bombus), when working on the smaller anthers in order to collect pollen, would, by moving the connective fork of the larger ones, press the apertures of the latter against the ventral side of its abdomen and powder it with pollen. Until now I have only seen a little fly (Syrphidæ) and *Trigona ruficrus* visiting this flower, both too small to fertilise it. The fly takes only notice of the yellow anthers; the Trigonas, too, always sit down first on these; but most of them (the more experienced specimens?) turn then round, and go to the larger anthers, which offer a more copious pollen-store, and work on them with their mandibles or eat them up entirely. Even if larger bees acted in the same manner as *Trigona ruficrus*, they would have powdered the ventral side of their abdomen before going to plunder the latter. The pollen of both kinds of anthers is white."

HERMANN MÜLLER

Palæolithic Implements in the Thames Valley at and near London. Their Comparative Numbers

IN my former letters, NATURE, vol. xxiii. p. 604, vol. xxiv. p. 29, I cited instances of the occurrence of these objects at great heights, indicating great antiquity, at the north and south of London. After the positions of the implements on the different old river terraces are considered, their numbers, as compared with the amount of material excavated, is a subject of considerable interest, as these numbers indicate in a broad way the amount of human population.

Before I give the results of my own experience I may say here that I have had these implements in view for about twenty years. I have not searched for them myself during all this time, although at first I commonly looked over pits and roads for implements and flakes with little or no result.

I had four reasons for beginning a thorough examination of the London gravels:—1. I had long taken a great interest in the subject. 2. I had particularly noticed the implement found in Gray's Inn Lane now in the British Museum, I had looked over Col. A. Lane Fox's collection from Acton and Ealing, and I knew of two implements from the gravels excavated near my own house. 3. I felt disappointed at not meeting with Thames valley implements myself. 4. I had been unwell through overwork, and my doctor told me I should not be well again till I regularly took a four-miles daily walk.

In the early spring of 1878 I determined to walk over the London gravels and note the constituent stones—not walk over the roads and pits once or twice, but ten, twenty, or if need be fifty times, so as to thoroughly acquaint myself with the stratification and materials.

I began in May, 1878, to examine the excavated gravel at Clapton, N.E. London, in the valley of the Lea. Here, after considerable searching, I found an implement and several flakes. I then mapped out the gravels for twenty-seven miles in a line east and west of North London, and wherever the gravel has been exposed in these twenty-seven miles I have been over it a great number of times. In three years—from May, 1878, to May, 1881, I found exactly one hundred implements, mostly lingulate examples (a few ovate), and thirteen trimmed flakes, *i.e.* genuine implements, but worked on one face only. This is equal to one hundred and thirteen perfect specimens. I also found twenty-one butt-ends and six points, some broken in Palæolithic times, others showing modern fractures; side-scrapers, six; flakes about one thousand four hundred; broken fossil bones, teeth, and tusks, chiefly mammoth and horse, not uncommon. Hammer-stones of quartzite, with abraded ends, none. An unabraded quartzite pebble, such as the pebble mentioned by Mr. Perceval, teaches nothing. Even if one end is abraded off, it might have been rubbed off by other pebbles passing over it whilst naturally fixed in the bed of a stream. When both ends of a quartzite pebble are abraded *quite away*, and the abraded parts are of a *distinctly different colour* from the rest of the pebble, such a stone is probably a hammer-stone. I have several genuine examples of these of Palæolithic age, but not from the Thames valley.

On reading these notes some persons may be inclined to exclaim, What a large number of implements! How common these objects must be! My reply is they are by no means common, but as a rule *extremely rare* and *most difficult to find*. One seldom sees a first-class implement resting flat and clean in the middle of a road or pit, inviting the passer-by to pick it up. They are usually half-buried, with only part of the point, edge, or butt visible, and that part frequently covered with clay or dirt, so that it requires a sharp and trained eye to distinguish the implements and flakes from the ballast with which they are incorporated.

My first attempts were to find how many implements occurred in a hundred tons of London gravel, but I found it impossible to determine this with certainty; I however could accurately find how many miles of the actual drift I had walked over, and my experience is that I walked in three years over four thousand five hundred miles of gravel to find one hundred and thirteen implements, equal to a walk of about forty miles for one implement.

Of course the implements may be more frequent in some places, as at Milford Hill, Salisbury, and Warren Hill, Mildenhall, and much less frequent in others, but the above statement is my personal experience in the twenty-seven miles of river-gravel to the north of the Thames at London. The men working in the roads and pits often questioned me, and I set all the men to look for the implements during my absence: the whole of the men together in three years produced twenty-two extra implements, ovate or lingulate, and worked on both sides.

The mere accumulation of implements was by no means my object. I felt from the first that to entirely depend upon workmen was a great mistake, as all ill-defined instruments must be lost. I therefore personally looked out for genuine new things, and especially wished to ascertain, if possible, what the implements themselves had to teach of the *men who made them*, how the implements were deposited, and if possible to calculate their age in years. With these objects in view I have kept a manuscript book, giving the exact circumstances of finding of every implement in my collection, not only in reference to the implements belonging to the Thames Valley, but to nearly all the implementiferous river-valleys of this country. With equal care I have kept a list of non-implementiferous positions, and my experience is, the lower gravels of the Thames as at Hammersmith and Battersea are barren. As soon however as a seventy or eighty feet terrace is reached, the implements and flakes crop up. Two implements have been found in the Thames at Hammersmith and Battersea, as recorded by Mr. Evans ("Stone Implements," p. 528), but these, of course, were washed out of a higher bed. I have found several flakes and an implement at Clapham Common and Battersea Rise, but here the heights are seventy to ninety feet. The most persistent searching at Lower Battersea and Hammersmith has produced with me absolutely nothing. With your permission I will give further results in a future letter.

WORTHINGTON G. SMITH

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THE COMET

WE have received the following further communications on the lately-visible comet:—

THE appearance of a large comet has afforded an opportunity of adding to our knowledge of these bodies by applying to it a new means of research. Owing to the recent progress in photography it was to be hoped that photographs of the comet and even of its spectrum might be obtained and peculiarities invisible to the eye detected. For such experiments my observatory was prepared, because for many years its resources have been directed to the more delicate branches of celestial photography and spectroscopy, such as photography of stellar spectra and of the nebulæ. More than a hundred photographs of spectra of stars have been taken, and in the nebula of Orion details equal in faintness to stars of the 14·7 magnitude have been photographed.

It was obvious that if the comet could be photographed by less than an hour's exposure there would be a chance of obtaining a photograph of the spectrum of the coma, especially as it was probable that its ultra-violet region consisted of but few lines. In examining my photographs of the spectrum of the voltaic arc, a strong band or group of lines was found above H, and on the hypothesis that the incandescent vapour of a carbon compound exists in

comets, this band might be photographed in their spectrum.

Accordingly at the first attempt a photograph of the nucleus and part of the envelopes was obtained in seventeen minutes, on the night of June 24, through breaks in the clouds. On succeeding occasions, when an exposure of 162 minutes was given, the tail impressed itself to an extent of nearly ten degrees in length.

I next tried by interposing a direct-vision prism between the sensitive plate and the object-glass to secure a photograph which would show the continuous spectrum of the nucleus and the banded spectrum of the coma. After an exposure of eighty-three minutes a strong picture of the spectrum of the nucleus, coma, and part of the tail was obtained, but the banded spectrum was overpowered by the continuous spectrum.

I then applied the two-prism spectroscope used for stellar spectrum photography, anticipating that, although the diminution of light would be serious after passing through the slit, two prisms, and two object-glasses, yet the advantage of being able to have a juxtaposed comparison-spectrum would make the attempt desirable, and, moreover, the continuous spectrum being more weakened than the banded by the increased dispersion, the latter would become more distinct.

Three photographs of the comet's spectrum have been taken with this arrangement with exposures of 180 minutes, 196 minutes, and 228 minutes, and with a comparison spectrum on each. The continuous spectrum of the nucleus was plainly seen while the photography was in progress. It will take some time to reduce and discuss these photographs and prepare the auxiliary photographs which will be necessary for their interpretation. For the present it suffices to say that the most striking feature is a heavy band above H which is divisible into lines, and in addition two faint bands, one between G and *h*, and another between *h* and H. I was very careful to stop the exposures before dawn, fearing that the spectrum of daylight might become superposed on the cometary spectrum.

It would seem that these photographs strengthen the hypothesis of the presence of carbon in comets, but a series of comparisons will be necessary, and it is not improbable that a part of the spectrum may be due to other elements. HENRY DRAPER

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MY first view of the comet was on June 25, when it appeared through a momentary opening in the clouds, with a nucleus that, in size and brightness, seemed to equal Venus at her best. The tail, immediately at its commencement, was exceedingly bright also, but I could see no more of it then, nor at a second view, also momentary, when little more than the head was visible. Notwithstanding the immense development of tail shown by the great comet of 1861 it could not compare for an instant in brightness with the nucleus of the present one as I saw it on June 25.

On June 27 I again observed it wonderfully waned in light, with a tail plainly traceable for ten degrees, and pointing directly to the Pole. The tail was slightly curved to the right; that is, it was convex to the apparent east, or on the preceding side, and was brightest and best developed on that side. There was no time for observation with the telescope when the clouds shut up the skies for the remainder of the night.

On the next night, the 28th, I got a view with the telescope, and by an observation, which was much interfered with by clouds, I made out the position to be, in Right Ascension, 5h. 58m. 44s., and in Declination 63° 12′ 53″, at 12h. 15m. Greenwich mean time. The comet was progressively waning, but the tail seemed still about 10° long, and pointed to the Polar star. The nucleus, though growing smaller, was still exceedingly brilliant, and as large as a star of the first magnitude. The direction of the tail did not differ very much from the comet's apparent course, and seemed *concave* to it, contrary to what is usually observed with other comets. With a power of 126 on a 4½-inch achromatic I saw a curve of light extending like wings on each side of the head, and outside, with a dark space between them, appeared a large enveloping curve of inferior brightness. I fancied at times that I could discern a very faint third envelope outside all. These curves extended farther in the direction of the tail on the *following* side than on the other, though it was on the latter or preceding side that the tail showed the best definition.

On July 8 the comet seemed much diminished in light and magnitude, though the nucleus was still brightly stellar. I could now see only the inner light-curve extending on both sides of the comet's head, and through it on the following side I distinctly observed a small star shining, as it would appear, with undiminished brightness. The preceding side of the tail was still brighter and better defined than the following. I made the comet's position at 14h. G.M.T. = Right Ascension 7h. 49m. 38s., and Declination 79° 27′ 21″.

On July 11 it was still further diminished, and in the bright moonlight there was no longer any light-curve traceable in my telescope. The nucleus however continued remarkably bright and star-like, and there was an extensive nebulosity round it. The tail might be traced by the naked eye three or four degrees. I had on this night a very satisfactory micrometrical observation, but I have not as yet been able to perfectly identify the star of comparison. The calculated light of the comet was scarce more than a tenth of what it was on June 22. It is now fast receding out of naked-eye view, and of little interest except for marking its position. This on July 20, at 12h. 20m. G.M.T., I made to be 11h. 17m. 47s. in R.A., and 82° 9′ 2″ in Declination.

Schæberle's comet is now well in view, and would be visible to the naked eye only for the brightness of the sky where it is moving. The weakest binocular is sufficient to show it, and it is rapidly gaining in brightness. On the 27th inst. it appeared to me with a stellar nucleus and a tail visible for about half a degree. I made its position at 14h. 15m. G.M.T. = 6h. 14m. 41s. R. Ascension, and 43° 59′ 10″ in Declination. The observation was difficult owing to the brightness of the sky and to clouds.

Millbrook, Tuam, July 28 J. BIRMINGHAM

A POPULAR ACCOUNT OF CHAMÆLEONS [1]

THE animal to which I propose especially to direct your attention to-day is one which has been the subject of many observations, and has inspired great interest from the most ancient times.

Its name "chamæleon" is derived from two Greek words signifying "Ground-lion," a name singularly inappropriate, since it is one of those creatures which are specially fitted by their organisation to live on trees, and which are comparatively ill at ease when on the surface of the earth.

It is by no means surprising however that this creature should have attracted the attention it has attracted, such is the singularity of its appearance and the peculiarities of its habits and properties. Neither is it surprising that it should have occasioned many errors and superstitions when we consider the erroneous beliefs current amongst ourselves with respect to our own toads and slow-worms, efts, &c.

Aristotle was acquainted (as was to be expected of him) with the singular motions of its eyes, but even he fell into some curious mistakes respecting it, and he tells us that

[1] Lecture delivered at the Zoological Gardens on July 28, 1881, by St. George Mivart, F.R.S.

it has no spleen and no blood except in the vicinity of its head and eyes.[1]

Pliny is careful to restate these errors, and further tells us that it lives without eating or drinking, and though generally an inoffensive animal, becomes terrible in the dog-days. He also adds,[2] on the authority of Democritus,[3] that it has the power of attracting to the earth birds of prey, so that they become in turn the prey of other animals, and that its head and neck, when burnt on oak charcoal, will cause thunder and rain to occur simultaneously. On the other hand, he rejects as fabulous the Grecian belief that its right leg cooked with a certain herb has the power of making a person invisible; that the thigh of its left leg mixed with sow's milk will induce gout if the foot be rubbed with the compound, and that a man may be made to incur the hatred of all his fellow-citizens by having his gate-posts anointed with a mixture of chamæleon's intestine and the renal secretion of an ape.

Aldrovandus informs us[4] (on the authority of older writers) that if a viper passes beneath a tree in the branches of which a chamæleon is perched, the latter will let fall some of its saliva upon the viper, which is thereby killed; and he further tells that elephants sometimes unwittingly eat chamæleons amongst the leaves of the trees on which they feed, and that the meal is a fatal one unless the elephants have recourse to the wild olive as an antidote. Gesner in his History of Animals[5] has carefully collected all these fables.

But though more accurate knowledge has dissipated many errors and destroyed many superstitions with respect to the chamæleon, yet such knowledge, far from detracting from the interest of our subject, has made it more than ever an object of scientific wonder and intelligent admiration.

My duty to-day, as I understand it, is to enable you to give a rational answer to the question, "What is a chamæleon?" and therefore to give you an accurate general notion of what the creature is in itself, and in what relations it stands to the world about it. Let us first look at the animal itself. It has a wonderfully lean and hungry look, and is in fact a hungry animal, and keen in pursuit of its insect prey at the present warm season. Its trunk is often greatly flattened from side to side, though sometimes swollen and inflated. It is never flattened from above downwards (as in so many lizards), but deep and raised up from the ground by the animal's long legs. Its head is large, and, in shape, somewhat triangular when seen in profile, and its upper surface is bounded on each side by a prominent ridge extending from the muzzle to the hinder part of the head or occiput. There is hardly any neck externally distinguishable. The limbs (of which there are two pairs) are long, uniformly slender members, each terminated by a paw in the form of a pair of pincers. There is a very long tail, also slender and curled towards its extremity, so as to be able to grasp firmly any object about which it may be rolled. The skin is rather soft and distensible. It is similar all over the body—not scaly (as in most lizards), but beset with small horny tubercles which become more close-set and flattened along the mid-line of the back and of the belly (where the tubercles project in a serrated manner), and also on the head. The mouth of the animal is very wide, but its lips meet so exactly that when closed the situation of the mouth is not readily distinguishable. The nostrils are small and open, one on each side of the muzzle, a little behind its apex. The eyes are very large, but the prominent eyeballs are covered by skin like that of the body, except at a minute central point where there is a small opening like an external pupil. Thus, instead of two eyelids, as in ourselves, there is one, formed as it were by the almost complete junction of two such as ours.

What is however much more remarkable than the form of the eyes is the manner in which they can be used. When we look at any object our eyes always move simultaneously, and are directed as much as possible towards the same object. We can thus make them converge, and we can restore the axes of our eyes to a parallel position, but we cannot make them diverge or direct one eye upwards and the other downwards at the same time. This limited power of motion in our eyes is with us innate and natural. Indeed such is the tendency to simultaneous action in our own eyes, that the very eyelids of our two eyes naturally move together, and it is only by repeated efforts that we obtain the power of moving them separately. The art of winking, then, is not an original gift but an acquired accomplishment, and this is especially the case with that refined winking which consists of a scarcely perceptible motion of the upper eyelid only.

In the chamæleon the motion of the eyes is not thus limited. It can move them with complete independence, and can simultaneously direct one eye upwards and forwards while the other gazes downwards and backwards. As far as I am aware, the chamæleon is the only animal which possesses this power.

But the chamæleon's eye has a very noteworthy internal structure. In ourselves the special organ of sight is an exceedingly delicate membrane called the "*retina*," which is spread out over the back of the inside of the eyeball. This membrane is composed for a part of its thickness of certain most minute structures termed "*rods* and *cones*," which are placed side by side, one end of each being directed outwards, and its other end towards the interior of the eyeball. At that part of the human retina which is directly opposite the pupil of the eye, is what is called "the yellow spot," which is the seat of our most acute sense of vision. In this yellow spot of ours there are many cones but few rods,[1] and the centre of it is formed of cones only. The cones of the yellow spot, moreover, are longer than those found in any other part of the retina.

According to Heinrich Müller, who has most carefully investigated the structure of this animal's eye,[2] the retina of the chamæleon has cones only, but no rods (like the centre of our yellow spot), while its cones are longer even absolutely (and therefore greatly longer relatively) than are our own. Finally the yellow spot itself is larger in the chamæleon than it is in us. Thus in all these respects the perfection of the human eye is exceeded by that of this very singular reptile.

That the chamæleon is able to gaze simultaneously at two distinct objects placed wide apart is not wonderful, because there are so many animals with eyes placed so completely on opposite sides of the head that many objects within the range of one of their eyes cannot possibly be seen simultaneously by the other. But even we are able to direct our attention simultaneously to two objects which lie towards opposite margins of our field of vision, while we neglect the sense impressions produced by all the various intermediate objects.

There is no external sign whatever of an ear in the chamæleon. Not only is there no projection, but there is no external aperture on the surface of the head, or any indication of the drum of the ear—an indication very commonly found in animals nearly allied to it. Nevertheless the chamæleon has a pair of ears substantially like those within our own skull, and these ears each communicate with the exterior by an aperture at the back of the mouth, as do ours also. It is this communication between the internal ear and the mouth which causes a man to open his mouth when he is intently listening.

[1] See his "History of Animals," book ii. chapter vii.
[2] See his book viii. Panckoncke's edition, Paris, 1830, vol. v. p. 318.
[3] See his book xxviii. chapter xxix.
[4] See his "De Quadrupedibus Digitatis viviparis," 1645, book ii. p. 668.
[5] See his "Historiæ Animalium," book ii. p. 2. The work was published in 1554.

[1] In the rest of the human retina the rods are much more numerous than are the cones.
[2] See *Würzburg naturwiss. Zeitschr.*, iii. 1861, pp. 10-42.

The chamæleon's internal ear however is not an exceptionally perfect organ like its eye. On the contrary, an important part, resembling a snail's shell in form, called the cochlea, which is largely developed in us, and which exists in a rudimentary manner in lizards generally, is absolutely and entirely wanting in the chamæleon.[1]

The tongue of this animal is the most wonderful of all its organs, and the chamæleon's entire organisation may be said to have been formed with reference to this most remarkable tongue.

If the animal's mouth be opened, its tongue will be seen as a thick fleshy mass lying between the two sides of the lower jaw. At the front end of this tongue is a cup-like depression with a prominence specially developed above and below it like an upper and lower lip. But this thick portion of the tongue thus at first visible is but a part of the entire structure. At its hinder end it suddenly narrows into another very long and cylindrical part, which is arranged and bent in transverse folds behind and beneath the thick part first described. This narrow part or, as it is called, "worm," finally bends to the front end of the lower jaw, where it becomes continuous with a third firmer part, which is rigid, because it contains a solid body within. This third part reaches from the front of the lower jaw to the back of the floor of the mouth, where it enters a sort of funnel-like depression, to the bottom of which it is firmly attached by flesh and membrane. The cavity of the mouth is very deep, as is necessary for the reception within it of this very voluminous tongue. When the tongue is elongated it may be extended six or seven inches. The action of the tongue will be spoken of in connection with what I have to say as to the other actions of the chamæleon.

The structure of the chamæleon's feet is very noteworthy. Each foot is (as has been said) practically a pair of pincers, but each branch of each pincer is made up either of two or of three toes bound together by the skin down to the very roots of the claws.

There are five toes or (as they are technically termed in anatomy) "digits" to each foot, and these five digits correspond with our own thumb and four fingers and our own five toes respectively.

In the fore-paw or hand of the chamæleon the digits which answer to our thumb, index, and middle digits are bound together in one bundle, while the digits answering to our ring and little fingers form the other bundle.

In the hind-paw or foot of the chamæleon the arrangement is different. There the digits which answer to our great and second toes are bound together in one bundle, and are opposed to another bundle formed of the third, fourth, and fifth toes.

Moreover while the three united digits of the fore-limb are directed inwards, the three united digits of the hind-limb are directed outwards.

There is yet another noteworthy point as to the structure of the paws. In ourselves the small bones which form our "wrist" and our "ankle" respectively are (as they are in almost all beasts) distinct and separate from those long, more or less slender bones which are in the palm of the hand and the sole of the foot, and which are called "metacarpal bones" in the hand and "metatarsal bones" in the foot. In the chamæleon however each metacarpal and each metatarsal absolutely unites with the wrist or ankle bone which is adjacent to it, so that they together form but one bone.

As to the internal organs of the chamæleon, I will only speak of the lungs. These organs are practically a pair of bags—air-bags—but each bag is furnished with seven or eight tubular prolongations, which seem each to end in a point. These ends however really open into certain sacs within the cavity of the body, which sacs can thus be inflated and the whole body much blown out.

[1] See Prof. Parker's paper in the *Transactions* of the Zool. Soc., vol. xi. p. 102.

The last structure I shall notice is the skin, so remarkable for the very conspicuous changes of colour it undergoes. The chamæleon's skin, like the skin of other animals, is furnished with very minute bags containing pigment. It is the presence of very many such bags containing a dark pigment which makes the negro's skin black. These pigment-bags are called "chromatophores," and the chromatophores of the chamæleon, unlike those of the negro, are contractile, and it is by the alternate contraction and expansion of chromatophores containing different coloured pigments that the changes of colour which take place in the chamæleon's skin appear to be effected.

The chamæleon does not make at all a bad pet. It is not only perfectly inoffensive, but most gentle and not at all wild, while it forms an object very interesting to contemplate. It needs to be kept warm and supplied with flies, mealworms, or other insects, and also with water, and with some branching shrub on which it may perch and climb. It is better to inclose the shrub in a glass case or cage, to prevent such accidents as happened to one of mine, which, being left alone and free, wandered to the fire-place, where it got beneath the grate, and so scorched its paws that it could no longer climb, and soon died.

Wonderful is the slowness with which the chamæleon ordinarily moves. When at rest it clings to the branches by its four paws and prehensile tail. When it wishes to advance it only moves one limb at a time. Let us say it begins by moving the right fore-limb. It first, of course, unhooks that paw, and then, bending the elbow, slowly raises it and holds it suspended a certain time, moving it right and left, forwards and backwards, till it finds a suitable foothold. Then its pincer-like fingers slowly and firmly grasp the new point of support, after which the left hind-limb performs a similar series of movements; then follows the left fore-limb, afterwards the right hind-limb, and finally the tail is unrolled, and then readjusted round some new sustaining object.

This is its ordinary mode of progress, but it can sustain itself by its tail only, and when thus hanging may seek for fresh foothold by stretching in various directions all its four limbs.

The chamæleon is probably the most thoroughly arboreal animal which exists. Many creatures of different kinds which live in trees are furnished with a prehensile tail. This is the case, for example, with the most arboreal monkeys, such as the spider- and howling-monkeys. It is also the case with that most arboreal member of the raccoon family, the kinkajou, and with the most arboreal members of the porcupine family and of the opossum order.

Arboreal animals may have their feet especially organised for climbing, as is the case with monkeys and opossums, but they are not such perfectly and exclusively climbing organs as are the chamæleon's feet. The sloths are animals the whole organisation of which is planned for tree-life, and their paws are modified to serve almost exclusively for climbing, and their digits are also bound together by skin to the roots of the claws. Moreover, in the sloths the wrist and ankle bones may more or less coalesce with the metacarpals and metatarsals, as in the chamæleon; nevertheless the sloth's digits are not pincers, but hooks only, all the digits of each foot being bound together in a single bundle. Moreover, admirable as is generally the arboreal organisation of the sloth, that animal is nevertheless devoid of a prehensile tail.

In birds the ankle-bones coalesce with the metatarsals, and there is a certain resemblance between the feet of the climbing arboreal parrots and those of the chamæleon, for though the parrot's toes are not bound together to the claws, they yet form a pair of pincers, two of them being turned in one direction and opposed to the other two Yet the mode in which they are grouped is different, for

in the parrot it is the first and fourth toes which are opposed to the second and third, instead of the first and second to the others.

In remarkable contrast with the slowness of its limb-movements is the quickness with which it can move its eyes, and above all its tongue. The chamæleon lives largely upon flies, and at first sight it would seem impossible that so apparently torpid and sluggish an animal should be able to reach and seize creatures not only active in their movements, but possessing the power of flight. At this season, when the chamæleon's appetite is keen, it may often be observed when a fly has been introduced into its cage to move about with comparative celerity, attentively watching the fly's movements, now with one and now with the other eye. It sooner or later happens that the fly settles for a few seconds somewhere within half a foot's distance of the chamæleon's head. Then the chamæleon's mouth may be observed to open and the apex of the tongue to protrude. In an instant it has shut again and the fly has disappeared. In fact the chamæleon has spit out, as it were, its enormously extensible tongue upon the insect, secured it by the viscid secretion with which the tongue is coated, and again withdrawn that organ together with the prey, but the whole has been effected with such amazing rapidity that the observer's eye cannot follow the movements of the reptile's tongue. It is projected and withdrawn without the slightest noise, but in the twinkling of an eye.

As I have said, it is this tongue which is as it were the centre of the chamæleon's organisation, and this tongue-movement is the very essence of its existence, and is its whole *raison d'être*. Without it the animal's life would be impossible, while the very slowness and deliberation of its other movements are a gain, since they enable the chamæleon to advance upon its prey within shooting distance without alarming it.

(*To be continued.*)

THE UNEXPLORED PARTS OF EUROPE AND ASIA

UNDER this title M. Venoukoff has just published an interesting paper on those parts of Europe and Asia which remain yet unexplored. It is not to be wondered at that the name of Europe should be among incompletely explored parts of the world, as there are even in Europe considerable spaces, especially in the Balkan peninsula and in North-Eastern Russia, which await scientific exploration. The war of 1877-78 certainly afforded occasion for surveying and mapping wide spaces in Bulgaria and Eastern Roumelia, but the geography of Macedonia, Epirus, and even of Thessaly is far from being exact. In Russia all the northern provinces, from the Norwegian frontier to the Ural Mountains are only known superficially; we know here only the coast and the three principal rivers—the Onega, the Dwina, and the Petchora. The great Samoyede *tundra* remains quite unexplored. Notwithstanding several journeys in the Northern Ural, this country is little known, and the interior of the great double island of Novaya Zemlya remains quite unknown, both affording, however, a very great interest, especially for geologists. As to the hydrographical exploration of the Kara Sea and of the Arctic Ocean north of Siberia, M. Venoukoff does not give them much of importance, notwithstanding what he terms the pompous newspaper writing about the trade with Northern Siberia, and he thinks that there are on the Asiatic continent several places far more interesting for explorers. For instance, Chekanovsky's and Nordenskjöld's explorations have quite changed our ideas on the geography of that land, twice as wide as France, which belongs to the basins of the Khatanga and of the Anabara. It would be a rich field of exploration for a bold traveller. The lands east from the Lena remain quite unknown, and the northern slopes of the Stanovoï Mountains are still a *tabula rasa;* the sources of the Indighirka, Kolyma, Omolon, Aniouy, and Ghijiga rivers were never visited by Europeans, and Wrangel mapped them only from hearsay. The land of the Chukchis is better known, thanks to the work of the explorers of the last century, to the recent Russian expeditions, and to Nordenskjöld's information; but all our knowledge of this country is far from being exact, and Europeans have never penetrated to the interior of the peninsula which separates the Arctic Ocean from the Pacific, and which promises to have a future as a meeting-point for the whalers, as well as for the trade in mammoth bones. The land of the Koriaks is less attractive, except for a naturalist. As to Kamchatka, certainly it is passably well known, but what a mass of work remains to be done in mapping the west coast, preparing a map of the interior, studying the most interesting geology, botany, and ethnography of the peninsula! Further south we see that the northern part of Sakhalin remains quite unexplored; the Sikhota-alin Mountains are all but unknown; and the regions between the Ussuri and Sungari Rivers, the sources of the Nonni and Argoun Rivers promise very much to the naturalist and to the geographer who would study them. The interesting peninsula of Corea will certainly be explored as soon as access to it is not forbidden to Europeans. In the Chinese Empire there are spaces as wide as England which remain unexplored. As to Eastern and Northern Thibet we are not yet sure as to what is the true source of the Brahmaputra and of the Irawaddi, and what is the importance in the orography of this land of the Kuen-Lun range. The inaccessible deserts of Eastern Turkestan are as deserving of exploration as Thibet, and the reaching of the sources of the Hoang-ho is one of the *desiderata* of geographical science. The great desert of Gobi is passably well explored, but still there remains an important problem: Does there exist, under the 42° and 43° N. lat., a chain of mountains which crosses the desert and unites the eastern Thian-Shan with the In-Shan Mountains? In northern Mongolia there still remain unknown the highlands at the upper parts of the Selenga River. In China proper there is certainly no room for geographical discoveries, but there remains very much to do as to astronomical determinations, and the substitution of a true picture of nature for the hypothetical chains of mountains which cover our maps. Useless to speak of what might be done with regard to the ethnography of Western and South-Western China. A most attractive exploration would be certainly that of Indo-China in all directions, but it is to be feared that such an exploration will remain for a long time a simple dream, because of the political institutions of this terrestrial paradise. But the exploration of Siam and Annam is one of the most necessary geographical *desiderata.* Without speaking of the Asiatic islands, where so much remains to do, M. Venoukoff points out that British India is certainly one of the best explored countries in the world, and that several parts of Europe are far behind India as to our geographical knowledge of them; but it is not the case as to those countries which are situated to the north-west of India. Afghanistan and Beluchistan await explorers, especially for certain, perhaps the most important, parts of them, as well as Southern Turkestan and the land of the Turkomans, where so much remains to do. Khorassan and Western Persia are quite well known, but Iran remains unknown; of course the exploration of these deserts, as well as of those of the interior of Arabia, would afford very great difficulties and give comparatively few scientific results. But a thorough geographical exploration of Armenia and of Asia Minor is most desirable; and, to finish with Turkey, M. Venoukoff asks if the Straits of the Hellespont and Bosphorus will

be seriously explored as to the most important question of the existence of an undercurrent in these straits?

As seen from this short sketch, there remains plenty of work for geographers and naturalists on our continent, and we may only express the wish that M. Venoukoff's idea of publishing a sketch of the "Unknown Lands," with a summary of the most important questions with regard to them, were executed on a larger scale, and that such a compendium were put into the hands of every young geographer.

NOTES

WE heartily commend to the attention of our readers the announcement of the Rolleston Memorial Committee, to be found in our advertising pages.

THE Scottish Zoological Laboratory, which last year supplied Mr. G. J. Romanes and Prof. Ewart with the material for their researches on Echinodermata, published in the Croonian Lecture, is this year to be placed at Oban. Those who intend to avail themselves of the advantages held out by this institution for the purposes of original work, are requested to communicate with Prof. Ewart, the University, Aberdeen.

THE accommodation for anatomical work at the Prosector's rooms in the Zoological Society's Gardens, which has hitherto been somewhat limited, has lately been increased by the erection of three new working-rooms, intended for the use of students. These rooms are now finished, and are at present tenanted by a small Long-Vacation class from Cambridge, who, under the superintendence of Mr. T. T. Lister, the Demonstrator of Comparative Anatomy in that University, are studying practically the anatomy of the *Mammalia* on the abundant material in that group provided by the Society's Menagerie. The class, it may be remarked, includes two lady students from Newnham College. It is to be hoped that when this class concludes its labours at the end of the month, other students may be found disposed to profit by the new facilities for work afforded them by the Zoological Society, and that thus the expense incurred in the erection of these new rooms may be fully justified by the increased scientific results reaped in the Regent's Park from the superabundant material at the disposal of Mr. Forbes.

THE honour of Knighthood has been conferred upon Mr. F. J. Bramwell, C.E., F.R.S., for his services to technical education.

THE German Emperor has conferred on Dr. Schliemann the Prussian Order of the Crown of the Second Class.

THE death is announced, at the age of seventy-seven, of Mr. Hewett Cottrell Watson, the well known English botanist.

DR. FERDINAND KELLER, the well-known Swiss archæologist, died at Zurich on July 21, in his eighty-first year. Dr. Keller had a decided taste for science in his youth, and shortly after leaving the University went to Paris, where he spent some time in the study of natural history. In 1832 he discovered a number of Celtic grave-mounds on the Burghölzli, a circumstance that led to the formation of the national Swiss Antiquarian Society. Of this Keller was named president, a position that he occupied for many years. The first work of the society was the exploration of the Burghölzli; and other similar researches, which threw much light on the primeval history of Switzerland, were undertaken. In 1837 began the publication of the Society's *Communications*, thirty volumes of which were wholly illustrated, and almost wholly written, by the president. In 1853 he opened the series of researches into the origin of Swiss lake-dwellings which have made his name so widely known, and revealed the way of life, in its minutest details, of a race of men whose mere existence had hardly before been suspected. Between 1860 and 1864 Dr. Keller gave to the world the results of his investigation of the Roman antiquities of eastern Switzerland. He wrote or edited further a history of the Abbey of Zurich, of the arms of Zurich, and sundry miscellaneous papers relating to life and culture in the middle ages. Until past his eighty-first year Dr. Keller continued to be an active and energetic member of the society which he had founded.

THE thirty-fourth summer meeting of the Institution of Mechanical Engineers was opened on Tuesday in Newcastle-on-Tyne, this being the third visit of the Institution to that town. The chair was occupied by the president, Mr. Edward A. Cowper. The president, after some remarks on foreign competition, gave a statement of the result of the progress of mechanical engineering during the past twelve years, and expressed the hope that the latter part of the present century would be marked not only by small improvements, but by many substantial inventions for the good of mankind. Mr. I. Lowthian Bell read an exhaustive paper on the Tyne as connected with the history of engineering, and a paper by Mr. F. C. Marshall of Newcastle, on the progress and development of the marine engine, was also read.

THE *Sydney Mail* brings us the welcome news that the Biological Station has been fairly established at Watson's Bay, under the direction of the well-known Russian naturalist, Dr. Miklucho-Maclay. During the last two years Mr. Maclay has been endeavouring to establish a zoological station in the neighbourhood of Sydney. Being seconded in his efforts by the Royal and Linnean Societies of Victoria and by the Royal Society of New South Wales, he has obtained from the Government an eligible site at Watson's Bay, most appropriate for the purpose. The station is situated on the shallow basin of Port Jackson, and close to the deep water of the Pacific, with large freshwater swamps and lagoons in the immediate neighbourhood, and a vast tract of wild forest country to the north, which probably will remain for a long time to come in its primitive wildness. The communication between Watson's Bay and Sydney by steamer being frequent and rapid (half an hour), the scientific work at the station will be greatly facilitated by the museums, gardens, and libraries of Sydney. The expenses for the building are estimated at 600*l.*, and will be covered by the sum of 300*l.* already allowed by Government and by the subscriptions, whilst the yearly expenses will probably be covered by annual grants of the Linnean and Royal Societies of Victoria and of New South Wales. The building, which stands on a slight eminence overlooking Camp Cove, with lovely views from the balconies on all the four sides, has been constructed to suit the requirements of a biological laboratory. It has five work-rooms, two bed-rooms, a bath-room, and a room for stores, plenty of light being provided for the work-rooms, which are fifteen feet by twelve feet each and twelve feet high. The partitions between each set of rooms are constructed of studding and double-lined, the space being filled with sawdust for the prevention of noise. The trustees are quite satisfied with the building; even upon so humble a scale as the present, it promises to give very good results. It may be hoped that the institution will become a bond of union between all those in Australia who are interested in biological research. As to the expected results we can but repeat M. Maclay's words: "Next after the tropics (which are the richest in animal life) the widest field offered to the investigator of nature, and consequently the most suitable region for the establishment of zoological stations, is Australia, with a fauna so interesting, so important, and so far from being sufficiently known, especially as regards anatomy and embryology."

PROF. C. V. RILEY, chief of the U.S. Entomological Commission, has accepted the position of Entomologist to the

Department of Agriculture, which has been tendered him by the new Commissioner, Dr. Geo. B. Loring. The appointment is to take effect on August 1. It will be remembered that, owing to differences with the retiring Commissioner of Agriculture, Prof. Riley resigned this same position two years ago, and we understand that he accepts it again at a salary less than that which the Members of the Entomological Commission get, because his reinstatement has been so generally demanded by scientific and agricultural associations, and because there is a near prospect, under the new administration, of the department being enlarged in scope and usefulness. Prof. J. H. Comstock, the retiring entomologist, was formerly an assistant under Prof. Riley, and will continue his connection with the department, pursuing the special work on the Coccidæ, which he has more particularly been engaged in. The reappointment of Prof. Riley will meet with approval not only in America, but here, where he has many riends and his work is well known and appreciated.

THE opening of the Exhibition of Electricity at Paris has been postponed as we anticipated. The ceremony will take place on August 11, and numerous speeches will be delivered by the public authorities. The electrical railway is being constructed from the Place de la Concorde to the Porte de l'Est of the Palais de l'Industrie. The posts required for supporting the copper wire required are attracting considerable public notice.

THE steamer *Travailleur* of the French Navy is now engaged in the Mediterranean Sea for dredging purposes, with a regular staff of scientific workers on board.

ONE of the results in Paris of the advent of two comets has been to infuse life into the popular Trocadero Observatory, which is visited by a large number of members, and where a course of lectures on several astronomical subjects is going on.

THE seventh annual conference of the Cryptogamic Society of Scotland will commence at Salen, Island of Mull, on Tuesday, August 30, 1881.

THE arrangements connected with the unveiling of the statue of Harvey at Folkestone on Saturday next, August 6, by Prof. Owen, are now nearly completed. It is expected that there will be a large concourse of doctors and others on the occasion. A small bust of the much-admired head of the statue is now on view in the western gallery of the Sanitary Exhibition at South Kensington, and can be had either in terra-cotta or imitation bronze. A reduction of the whole statue is also contemplated by the sculptor, Mr. A. B. Joy, and will be completed if a sufficient number of subscribers should order it.

IN a *Gazette* covering 250 pages have been published the new Statutes which have been promulgated by the University of Oxford Commissioners.

WE have received *Anthony's Photographic Bulletin* (New York) for June, containing an enlarged copy of Dr. Henry Draper's photograph of the nebula in Orion. The enlargement shows we have very little to hope for in this direction, still the result is a *tour de force* which reflects credit on Dr. Draper. The same number contains an excellent photograph of the Doctor himself.

WE regret to learn that the Committee, formed more than twelve months ago, to raise and present to Dr. William Farr, C.B., F.R.S., a testimonial on his retirement from the public service, have only succeeded in obtaining 930*l*. This sum has been temporarily invested in the names of trustees; and, disappointed as the Committee feel at the comparatively small success of their efforts, they have decided to close the fund so soon as they are able to obtain the small balance now required to raise the amount of the testimonial to 1000*l*. We trust there will at least be no difficulty in obtaining the small sum still required to complete the testimonial.

PHILOLOGISTS will be glad to learn that Prof. G. Beltrame's valuable papers on the Denka language have at last been published in full. They occupy the whole of the current volume of the Italian Geographical Society's *Memoirs*, and consist of three parts, a very complete grammar, an Italian-Denka and a Denka-Italian vocabulary, the former of nearly 4000, the latter of 2000 words. The grammar had already appeared in previous bulletins of the society; but these are now mostly out of print, and in any case the directors rightly considered that students would find it convenient to have all the documents collected in one volume. The Denka is one of the most widespread as well as one of the most interesting of all the Negro tongues current in the White Nile region, being spoken with great uniformity by all the tribes between 5°–12° N. along the main stream and its tributaries, who are collectively known to the Arabs as the Denka nation, but who call themselves *Jen*, a derivative form of *Jan* = race, people. They lie mainly between the Nuër and Shillûks on the north, and the powerful Bari nation on the south, stretching westwards as far as Dar-Fertit, and south-westwards to the Nyam-Nyam country. There are altogether twenty-five chief tribes, but the common speech presents scarcely any dialectic variety except amongst the Shîr in the extreme south, and amongst the Abuyo and others in the Sobat valley. The language itself is quite distinct from any of the other Upper Nilotic idioms, and is characterised by remarkable regularity and clearness in its structure. It is entirely destitute of grammatical endings, and most of the words are monosyllabic. Prof. Beltrame belongs to a somewhat obsolete school of philologists; hence still speaks of six cases, moods, and other verbal forms. But it is sufficiently evident from his otherwise lucid exposition, and especially from his copious examples, that there are neither cases, moods, tenses, nor, strictly speaking, verbs at all in the language. It need scarcely be remarked that the Denka has nothing in common either with Galla, Ki-Ganda, or other members of the surrounding Hamitic and Bantu linguistic families. It forms one of the numerous independent groups that have been developed during the course of ages amongst the true negro tribes of Sudan and the Upper Nile Valley. Prof. Beltrame's papers must be regarded as a valuable addition to our knowledge of African forms of speech, and will prove of permanent value when the time comes for a more exhaustive study of this ethnical domain.

THE International Pharmaceutical Conference is holding its meetings in London during the present week; one of the questions engaging its attention is an International Pharmacopœia, the desirability of which is generally admitted.

A SLIGHT shock of earthquake was felt at Bangor, Maine, U.S., on July 31.

AT the last meeting of the Natural History Class of the University of Edinburgh, held on Monday, July 25, an illuminated address was presented to Prof. Alleyne Nicholson, of St. Andrews, who has been lecturing on behalf of Sir Wyville Thompson for the past three sessions. The address was signed by about 500 of the students who have attended Prof. Nicholson's lectures during the past three years.

SHORTLY before midnight on July 20 a splendid meteor was observed at Munich. It resembled a fiery ball of 30 centimetres diameter, and it passed slowly from south to north in an almost horizontal direction.

AT the distance of twenty-seven miles north-east from Padang (on the western coast of Sumatra) and some fifteen miles east from Lake Singkarah, we find a high land very similar to the Saxon highlands, and reaching a height of 2400 feet above

the sea-level, the hills of which are formed of sandstones which contain immense coal-fields. According to a description of them, just published by D. D. Veth in the *Deutsche Geographische Blätter*, these coal-fields may contain altogether no less than 300 millions of tons of good coal. The northern, or Parambahan part of them contains two main beds of coal, having an average thickness of thirty-three feet and occupying a surface of about three square kilometres, that is, about 20 millions of tons of good coal; but the rocks are rather disturbed, and therefore the extraction of coal would be difficult. The middle, or Singalut part, situated on the right bank of the Ombilin River, contains about 80 millions of tons of coal, and consists of seven thin beds of coal, which have altogether an average thickness of 16 feet, But the best coal-field is the southern, or Sungei-Durian part. situated on the left bank of the Ombilin River, which contains about 200 millions of tons of good coal. The beds of coal are three, having a thickness of 20, 7, and 7 feet, separated from one another by sheets of sandstone 50 to 70 feet thick. As to the quality of the coal, thirteen tons having been extracted and brought to Padang, it was found that as fuel for steam-engines this coal is not below that of Cardiff or Newcastle, but that it would not be as good as these two in the production of lighting gas or for iron furnaces. As to the transport of this coal to the sea-coast, it would necessitate the construction of a railway 65 or even 100 miles long.

THE Danzig *Naturforschende Gesellschaft*, which numbers now no less than 398 members, has just issued a new volume of its *Proceedings* (new series, vol. v., fascicules 1 and 2). It contains, besides the minutes of meetings of the Anthropological, Physical, Chemical, and Medical Sections, much valuable information, especially as to the botany and zoology of Prussia. The *pièce de résistance* of this volume is an essay at a topographical flora of West Prussia, by H. von Klinggraeff, being a *résumé* of the author's own researches and of what is known on the flora of this province. The author finds that there are in this province no less than 1218 species of Phanerogams, 44 species of cellular Cryptogams, 363 species of mosses, 18 of Characeæ, and 276 species of lichens, and he takes into account only the true inhabitants of the province. As to the lower Cryptogams, the figures are but provisional ones, as the algæ and mushrooms of the province are but incompletely known. We notice also in this volume papers on the freshwater molluscs of the neighbourhoods of Danzig, by E. Schumann; on the Ichneumonids of Western and Eastern Prussia, by C. Brischke; the *Reports* on the third meeting of the Botanical and Zoological Society of Western Prussia, containing a series of catalogues of plants found during botanical excursions; an interesting paper by C. Brischke, which deals with a rather neglected question, namely, with the Phytophags which the author has observed and cultivated in the neighbourhood of Danzig; a paper on the bronze-basin of Steinwage, by Dr. Fröling; and on the Cenoman fossils which are found in the diluvium near Danzig, by Dr. Kiezow.

THE St. Petersburg Naturalist's Society intend to offer various prizes for botanical papers, and to couple with them the name of the late Dr. Schleiden, who was a member of the St. Petersburg Academy and Russian State counsellor.

ON July 21 the meeting of Polish Naturalists and Physicians took place at Cracow. Some 500 members attended the meeting.

ACCORDING to the latest investigations the *Phylloxera vastatrix* has spread enormously upon the peninsula of Istria, particularly in the neighbourhood of Pirano. The plague threatens to infect the vineyards of the Karst, of Friaul, and of Carniola.

WE learn from a circular, issued by the Director of the St. Petersburg Central Physical Observatory, that all the Arctic meteorological stations will soon be opened, and that about the autumn of 1882 we will have observations from these stations for a whole year. The following, we may remind our readers, are the stations to be established:—At Upernivik, by Denmark; in Northern Finnmarken, by Norway; on the Jan Mayen Island, and, if possible, on the western coast of Grönland, by Austria-Hungary; on Spitzbergen, by Sweden; on Novaya-Zemlya (already opened a year ago) and at the mouth of Lena River, by Russia; on Point Barrow and in Lady Franklin's Bay, by the United States. Sites have already been taken by the United States and Norway to open new stations. It is to be hoped that meteorological stations will be opened, according to the wish of the International Conference at Bern, also in Antarctic regions, namely, on South Georgia, by Germany, and at Cape Horn, by France; whilst the Netherlands expect to establish a station further in the Arctic region, namely, at Dickson Haven in Siberia. The International Conference which will be opened at St. Petersburg will establish the method of observation to be adopted at all these stations.

AN International Exhibition is planned for 1883 at Shanghai.

THE additions to the Zoological Society's Gardens during the past week include two Common Marmosets (*Hapale jacchus*) from South-East Brazil, presented by the Lord W. G. Cecil; two Common Squirrels (*Sciurus vulgaris*), British, presented by Mr. C. B. Barber; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. Douglas; two Common Jays (*Garrulus glandarius*), British, presented by Mr. Arthur F. Astlay; a Common Cuckoo (*Cuculus canorus*), British, presented by Mr. Harry Morrisson; a Surucucu Snake (*Lachesis mutus*) from Pernambuco, presented by Mr. C. A. Craven; two Common Boas (*Boa constrictor*) from South America, presented by Mr. G. H. Hawtayne; a Common Adder (*Vipera berus*), British, presented by Mr. J. Snow; two Blossom-headed Parrakeets (*Palæornis cyanocephalus*) from India, four Common Widgeons (*Mareca penelope*), an Osprey (*Pandion haliætus*), European, purchased; a Guinea Baboon (*Cynocephalus sphinx*) from West Africa, received in exchange. Amongst the additions to the Insectarium during the same time are imagos of *Antheræa yama-mai*, bred from eggs, and larvæ of the Lobster Moth (*Stauropus fagi*), Pebble and Swallow Prominent Moths (*Notodonta ziczac* and *dictæa*) and Purple Thorn Moth (*Selenia illustraria*). Numerous Ant-Lions (*Myrmeleo formicarius*) are also now emerging in the perfect state from their burrows in the sand.

SOLAR PHYSICS—THE CHEMISTRY OF THE SUN[1]

WHAT then are those precise difficulties to which reference has been made?

The number of them is considerable, and they have arisen from careful study extending over many different fields of work.

1. We most conveniently begin by noticing those suggested in the work of comparing the lines of the different elementary bodies with the Fraunhoferian lines; work done chiefly by Kirchhoff, Ångström, Thalén and others. Kirchhoff was not long before he found that to say that each substance had a spectrum entirely and specially belonging to that particular substance was not true. He says,[2] "If we compare the spectra of the different metals with each other, several of the bright lines appear to coincide." Now Kirchhoff was working with Bunsen as his collaborateur, and therefore this was not said lightly, as we may imagine. Similarly Ångström, who was working with the assistance of the Professor of Chemistry at Upsala, was driven to exactly the same conclusion. He says[3]—

[1] Lectures in the Course on Solar Physics at South Kensington (see p. 150). Revised from shorthand notes. Continued from p. 301.

[2] "Researches on the Solar Spectrum." Roscoe's translation. Part I. p. 10.

[3] "Recherches sur le Spectre Solaire," p. 36.

I translate his words—"Of all the bodies iron has certainly produced the greater number of lines in the solar spectrum. Some of these seem to be common with those of calcium." Thalèn carried on this work, and if one compares the magnificent tables, which we owe to his untiring skill and industry, one is perfectly astonished to find the number of coincidences which he has so carefully tabulated.

2. There was another kind of work, a newer kind of work, going on. Observers began to give particular attention to the bright lines of flames, and the lines thickened in spots. And here I may limit myself to the general statement that the divergence between the spectra of the different substances as observed in the sun and in our laboratories was very much intensified as facts were accumulated. Very many of the lines observed in flames were lines with no terrestrial equivalents, and the spot-spectrum often contained lines much thickened, which were either not represented at all, or only feebly among the Fraunhofer lines.

3. Next, among all the metalloids known to chemists only one of them—or one substance classed as such, hydrogen—was present in the solar atmosphere, and that in overwhelming quantity; whereas the efforts of Ångström, Kirchhoff, and others could not trace such substances as oxygen, chlorine, silicon and other common metalloidal constituents of the earth's crust.

4. Then again, the layer which was produced by what was taken to be gaseous magnesium round the sun, a layer indicated by the brightest member of the *b* group, was always higher—always gave us longer lines—than that other layer which was brought under our ken by the bright line D seen in the spectrum of sodium.

Here was a distinct inversion of the chemical order. The atomic weight of sodium being 23, and of magnesium being 24, the sodium ought to have been higher than the magnesium; but the contrary was the fact, and that fact still remains after twelve years of observation.

5. As the work of tabulating the lines went on, and the more complex outpourings of vapours from the sun's interior were studied, it was found that the lines of iron, calcium, and so forth revealed to us were by no means the brightest lines—by no means the most important, or most prominent lines, but lines which really we had very great difficulty in recognising as characteristic of any particular spectrum. There they certainly were, however, mapped us very fine lines by the most industrious observers. Similarly with the spots, there was an absolute inversion of the thickness of the lines of any one substance in the spot. Surely there was a great screw loose here.

6. Closely allied to these observations we had another extraordinary fact. We could quite understand why in a spot the change of refrangibility of the magnesium lines when there was a storm going on in the sun should be different from the change of refrangibility of, say, the iron lines. The natural explanation was, of course, this: you have the magnesium gas going at one rate, the iron gas going at another rate, and that is all there is to be said about it. But it was soon found that the differences which could be sharply seen between the spectrum of a particular mass of magnesium vapour and a particular mass of iron vapour extended to the iron vapour itself. There were just as many variations in the refrangibility of the lines of iron itself, for instance, as there were between the lines of iron and other substances: that is to say, we had in the one case magnesium going at one rate and iron going at another rate; but when we came to deal with the iron lines alone we found one iron line told us the iron vapour was going at one rate, and another iron line told us that same iron vapour was going at another rate. It will be seen at once that there was a great difficulty in that.

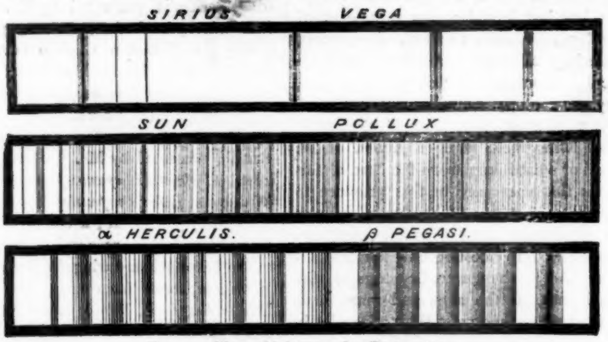

FIG. 27.—Three chief types of stellar spectra.

7. Further. The lines on which these observations of the relative motions of the vapour depended were found to go in sets. In a spot, for instance, we would generally see movement indicated by one set of iron lines, whereas in a prominence we would always see a different set—a set in a different part of the spectrum altogether—registering this movement for us. Here again was considerable food for thought.

That was stated very roundly a good many years ago—in 1869. I will read what was then written on this subject:[1] "Alterations of wave-length have been detected in the sodium, magnesium, and iron lines of the spot's spectrum. In the case of the last substance the lines in which the alteration was detected were not those observed when iron, if we accept them to be due to iron alone, is ejected into the chromosphere."

That caveat with regard to iron arose from the fact that of the 460 lines recorded by Kirchhoff in 1869 only three lines of iron had been seen bright in the solar prominences.

8. Then came a point which has been very slightly alluded to already. How came it that the total chemical composition of this atmosphere of the sun, which we were taught to look upon as the exemplar of what must have once happened to our own planet, varied so enormously from the composition of the crust of our earth? No oxygen in it, no silicon, no fluorine; whereas we get abundance of titanium, nickel, and so on. It was difficult to imagine a stronger difference to exist between any two masses of matter than the chemical constitution of the incandescent sun, and of the earth, which is now cooling.

9. There was still another point of view very soon forced upon solar observers by the magnificent success which had attended the labours of Dr. Huggins, Secchi, and other observers in recording the spectra of stars. It was a most interesting inquiry naturally to see whether the stars gave spectra quite like each other, and if it should happen that they did not give spectra like each other, then the points of difference would be sure to give us some excellent working suggestions.

Now what are the facts? Here are three typical stellar spectra (Fig. 27), which show us at once that there is a very considerable difference in the phenomena. In the upper part of this diagram we have a star remarkable for the fewness of lines in its spectrum. From one end of the spectrum to the other there are not above half-a-dozen prominent lines. In the next part however we have a star which is remarkably like our own sun, both as regards the number of lines and their arrangement. In the lower part of the diagram, on the other hand, we have a star in which we get flutings instead of lines; so that we get not only a difference of degree, but a fundamental spectroscopic difference of kind. Now there is a circumstance connected with that first star with the simple spectrum very striking to any one in the habit of observing the sun, and it is this: those lines visible in the star, which, be it remembered, had been independently determined to be hotter than our sun, are precisely those lines, and none other, which we see bright on the disk of the sun itself. I have emphasised the fact that

[1] *Proc.* Roy. Soc., vol. xviii. p. 74.

we have independent evidence that the star with very few lines is hotter than our sun. It is also clear that the other star with the fluted spectrum is a star much cooler than our sun, because it was one of those red stars, the light of which is exceedingly feeble, which, on grounds independent altogether of spectroscopic evidence, are supposed to be stars in the last stage of visible cooling.

So much then for some of the earlier observations on the coincidence of metallic lines in the sun, with observations on the lines themselves in different portions of the sun's atmosphere.

10. We now come to another part of the work where we also find difficulties. Ångström, in that exceedingly important memoir which accompanies his Atlas, states:[1] "In increasing successively the temperature I have found that the lines of the spectra vary in intensity in an exceedingly complicated way, and consequently new lines even may present themselves if the temperature is raised sufficiently high." Kirchhoff, on his part, had seen phenomena very similar to those thus touched upon by Ångström, but his explanation was a different one. He did not agree that the temperature upon which Ångström laid such strong stress was really the cause at work. He attributed those changes rather to the mass and the thickness of the vapours experimented upon—nay, he went further: at a time when scarcely any facts were at his command he broached a famous theorem which went

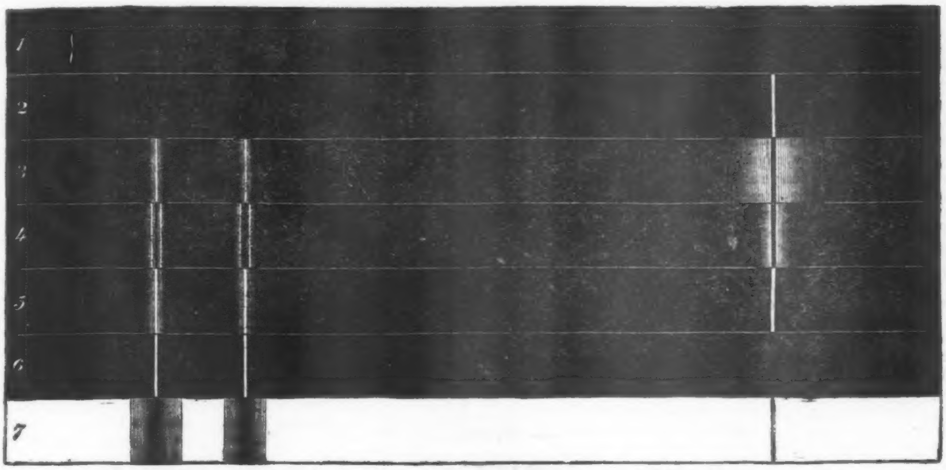

FIG. 28.—The blue end of the spectrum of calcium under different conditions. 1. Calcium combined with chlorine ($CaCl_2$). When the temperature is low, the compound molecule vibrates as a whole, the spectrum is at the red end, and no lines of calcium are seen. 2. The line of the metal seen when the compound molecule is dissociated to a slight extent with an induced current. 3. The spectrum of metallic calcium in the electric arc with a small number of cells. 4. The same when the number of cells is increased. 5. The spectrum when a coil and small jar are employed. 6. The spectrum when a large coil and large jar are used. 7. The absorption of the calcium vapour in the sun.

to prove this; and yet what had Kirchhoff himself done? how had he traversed his own theory? He states that his observations were made by means of a coil using iron poles one millimetre in thickness. Now the thickness of a short spark taken from iron poles one millimetre in thickness would probably be two millimetres. Next Kirchhoff allocated the region where the absorption which produces the reversal of the iron lines took place at a considerable height in the atmosphere of the sun, and he expected the atmosphere of the sun to be an enormous mass represented by the old drawings of coronas, so that on Kirchhoff's view the thickness of the iron vapour which reversed the iron spectrum must have been, at a moderate estimate, 10,000 miles, and yet he said that the spectrum of that, and of the light given by the coil were absolutely identical; that is to say, that the *fact* was that the variation of thickness from two millimetres to 10,000 miles made no difference. That was on the one hand; on the other hand he gave us his *theorem*, showing that a slight variation of thickness would produce all the changes which Ångström and others had observed up to that time, and which we have observed since in much greater number.

A diagram (Fig. 28) will show the sort of changes to which Ångström referred, changes which have been observed by every

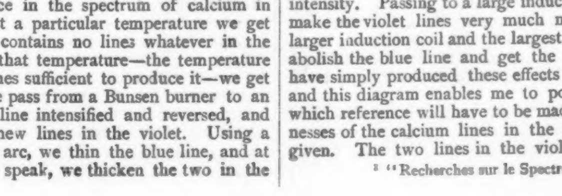

FIG 29.—Fluted spectrum of iodine.

new worker who has taken up the subject. It represents the variations which take place in the spectrum of calcium in the photographic region. At a particular temperature we get a spectrum of calcium which contains no lines whatever in the blue, but when we increase that temperature—the temperature of a Bunsen burner is sometimes sufficient to produce it—we get a line in the blue. When we pass from a Bunsen burner to an electric lamp we get this blue line intensified and reversed, and at the same time we get two new lines in the violet. Using a still higher temperature in the arc, we thin the blue line, and at the expense of that line, so to speak, we thicken the two in the violet, so that the latter equal the blue line in thickness and intensity. Passing to a large induction coil with a small jar we make the violet lines very much more prominent, and using a larger induction coil and the largest jar we can get, we practically abolish the blue line and get the violet lines alone. Now we have simply produced these effects by varying the temperature, and this diagram enables me to point out one of the things to which reference will have to be made subsequently. The thicknesses of the calcium lines in the spectrum of the sun are also given. The two lines in the violet are really H and K. The

[1] "Recherches sur le Spectre Solaire," pp. 38, 39.

other line—the all-important one at low temperatures—is feeble and unimportant. So that both on the solar evidence and on the evidence of all these spectra, whatever the explanation may be, there is the undoubted fact that fundamental changes of intensity in the lines are produced by some cause or other, and if Kirchhoff's statement about the matching of lines is true for one temperature it is false for all the others.

II. In my reference to stellar spectra I mentioned the word "fluted" spectrum. Before Kirchhoff had published his first paper two very eminent Germans—Plücker and Hittorf—were working at spectrum analysis at Bonn, and they found that in the case of a great many simple substances what are called fluted spectra were to be observed as well as line spectra.

The accompanying diagram (Fig. 29) of the fluted spectrum of iodine will show the difference between these fluted spectra and the line spectra, on which we have been exclusively occupied up to the present.

We observe that the chief novelty is an absolute rhythm in the spectrum; instead of lines irregularly distributed over the spectrum, we have groups which are beautifully rhythmic in their structure. The next diagram (Fig. 30) shows us the radiation spectrum of a particular molecular grouping of carbon vapour, that also is beautifully rhythmic; the rhythm of each of the elementary flutings exactly resembling that of the iodine.

These observations were among the first to suggest the idea that the same chemical element could have two completely distinct spectra. They were eminently suggestive, for if two, why not many?

In my reference to the "long and short" method of observation I stated that it enabled us to note what happens when a known compound body is decomposed. With ordinary compounds, such as chloride of calcium and so on, one can watch the precise moment at which the compound is broken up—when the calcium begins to come out; and we can then determine the relative amount of dissociation by the number and thickness of the lines of calcium which are produced. Similarly with regard to these flutings we can take iodine vapour, which gives us this fluted spectrum, and we can then increase the temperature suddenly, so that we no longer get the fluted spectrum at all, or we may increase it so gently that the lines of iodine come out one by one in exactly the same way that the lines of calcium came out from the chloride of calcium. We end by destroying the compound of calcium in the one case, and by destroying the fluted spectrum in the other, leaving, as the result in both cases, the bright lines of the constituents—in the one case calcium and chlorine; in the other case iodine itself. I have by no means exhausted the list of difficulties which were gradually presented to us when we considered that both in the sun and in our laboratories spectrum analysis brought before us the results of unique, absolutely similar "chemical atoms." Not only were there differences, but the differences worked in different ways, whether we passed from low to high temperatures in laboratory work, or from the general spectrum or the flame spectrum in the sun.

But I have said enough for my present purpose; details on the points I have referred to and on others must be gone into afterwards.

How then was one to attempt to grapple with these difficulties? Was it the time to found new theories? or to rest and be thankful? Was it not better to appeal to what was known—to proceed in accordance with Newton's laws of philosophising, and start no new principle unless one were absolutely bound to do so: to appeal in fact to the law of continuity, and to suppose that the explanation of a very large part at all events, of this new matter, lay in the fact that, all unconsciously, spectroscopists had been working under more transcendental conditions as regards temperature than had ever been employed before, and that the natural result was that this higher temperature had done for the matter on which they had experimented exactly what all lower temperatures had been found to do. That is to say, that they had been broken up. In other words, it lent great probability to the view that when we subjected, say iron—because it is a good thing to keep to one specific substance—to one of these transcendental temperatures, we were no longer dealing with the spectrum of iron, but with the spectrum of the constituents of iron revealed to us by a temperature at which no experiments had been made before.

And one was the more struck by the probability of this being at all events an approximation to the truth by those stellar spectra to which I have referred, and by the knowledge we possessed, that in the case of a star of the simplest spectrum we were dealing with the highest possible temperature. So the idea was thrown out that these stars were really simpler in their structure; that their immense temperature had not allowed a complex evolution of higher complex forms of chemical matter to take place; and that we had there the primordial germs of matter, so to speak, or at all events something nearer to the beginning of things than anything that we had in this cool planet of ours, or anything that we were likely to find easily here, in consequence of the various difficulties which harass every kind of experimentation. It was imagined that we might picture to ourselves a sort of celestial dissociation in the heavenly bodies which would place those stars, the spectra of which have been seen, in a different order; that the first star with lines should be a star of the simplest spectrum, the next star with lines should be that which mostly resembled our sun, and that the last in order should be that one in which the lined spectrum had utterly disappeared in favour of the fluted spectrum. If this were granted for the stars, why not attach all this to the sun? Because, as has already been mentioned, all these lines which were seen in the spectra of the hottest stars were precisely those lines which were seen most intense in the hottest parts of the sun; and it did really seem as if in that way we could eventually sooner or later—most likely later, for Art is very long—get some light on the subject.

I at once say that this idea which was thrown out in the year 1873 on spectroscopic evidence had been anticipated by the foremost philosopher amongst English chemists of his time; I mean the late Sir Benjamin Brodie.[1] From considerations of a perfectly different kind he had come to the conclusion that our chemical philosophy was not anything like so firmly based as was generally imagined, and that, given a higher temperature, the elementary bodies would cease to be elementary—that the adjective "elementary" applied to them was merely the measure of our inability to dissociate them; and to watch the progress of dissociation when we got them at a temperature at our command. By a stroke of genius he, before anything was known about the chemistry of the sun, went to the sun for that transcendental temperature he was in search of; thus showing that he had an absolutely pure and accurate conception of the whole thing as I believe it to be—but that is anticipating matters. He suggested that the constituents of our elementary bodies might be found in the hottest parts of the solar atmosphere existing as independent forms. The whole merit of that conception therefore is due to Sir Benjamin Brodie, and dates from the year 1867.

Now we can easily understand, seeing that much of the spectroscopic work which had been done up to 1874 had had for its object the connecting—intermingling, so to speak—of solar, stellar, and terrestrial chemistry, that it was not a pleasant thing to find that the path seemed about to be such a very rugged one—that we seemed after all not to be in the light, but in the dark, and the very practical question was, what was to be done? Would it have been wise to have considered, *then*, the whole question of the dissociation of elementary bodies? I think it would not have been wise; the data were insufficient. The true thing to be done was, I think, to endeavour to accumulate a vast number of new facts and then to see what would happen when a sufficiently long base of facts had been obtained. What did we want? We chiefly wanted to settle those questions of the variations of spectra seen in our laboratories, and the variations observed when we passed from the spectrum, say of iron on the earth, to the spectrum of iron in solar spots and storms. The coincidence of lines of different bodies which had been referred to by Ångström and Kirchhoff also required investigation. What more ready means of doing that—what more perfect means were there than those placed at our disposal by photography? Photography has no personal equation, it has no inducement to cook a result either in one direction or the other, and it moreover has this excellent thing about it, that the results can be multiplied a thousandfold and can be recorded in an absolutely easy and safe manner. There were other reasons why photography should be introduced. We see at once that it was quite easy to introduce the process of purification of the spectra to which I have already drawn attention, by merely comparing a series of photographs; the A, B, C of my diagram (Fig. 26) being represented, say, by iron, cobalt, and nickel, or any other substances. Again, it was quite possible by the use of the electric lamp to very considerably increase the

[1] "Ideal Chemistry." Lecture delivered to the Chemical Society in 1867, republished 1880. (Macmillan).

dispersion which Angström had employed; so that, if impurities had been suggested, there was now a method which has not yet been challenged of getting rid of them. If the dispersion was then insufficient there was nothing to prevent it being made very much more considerable, because a perfect photograph will bear a very considerable amount of magnification.

The diagram (Fig. 31) will show the method of photography that was adopted in this work, and by which the various photographs thrown on the screen were taken. The object was to compare the light of the sun with the light of the vapour in the electric arc of any particular substance that we wished to observe. By means of a heliostat and lens an image of the sun was thrown exactly between the poles of an electric lamp, and the rays diverging from it were collected by a second lens and again brought to a focus, this time on the slit of the spectroscope. The slit was provided with two slides, by means of which either its upper or lower half could be exposed, while the other half was covered. If we wished to take the solar spectrum first, the

Ultra-violet fluting. Blue fluting.

FIG. 30.—Carbon flutings, contrasted with the line-spectra of calcium, iron, aluminium, and other impurities of the poles.

poles were separated so that they might not obstruct the sunlight; the image of the sun was allowed to fall on one-half of the slit, and the plate was exposed. That half of the slit was then covered up and the other half opened (the sunlight being cut off), and the substance volatilised in the electric arc so that its image fell on the open part of the slit. The plate was again exposed, and so the two spectra were obtained, one above the other. In this way then we had, first of all, a spectrum of the sun compared with the spectrum of the particular substance we wished to map.

After that we had the long and short lines in the same substance photographed on another plate. After that we had all the substances which might exist as impurities in the first substance—that is to say, all the chemical elements photographed with their lines—their long and short lines, in precisely the same manner; and finally we had a comparison of the substances we wished to photograph, say iron, with a spectrum of every other substance which might contain these impurities. It will be seen therefore that an enormous number of photographs had to be taken. As

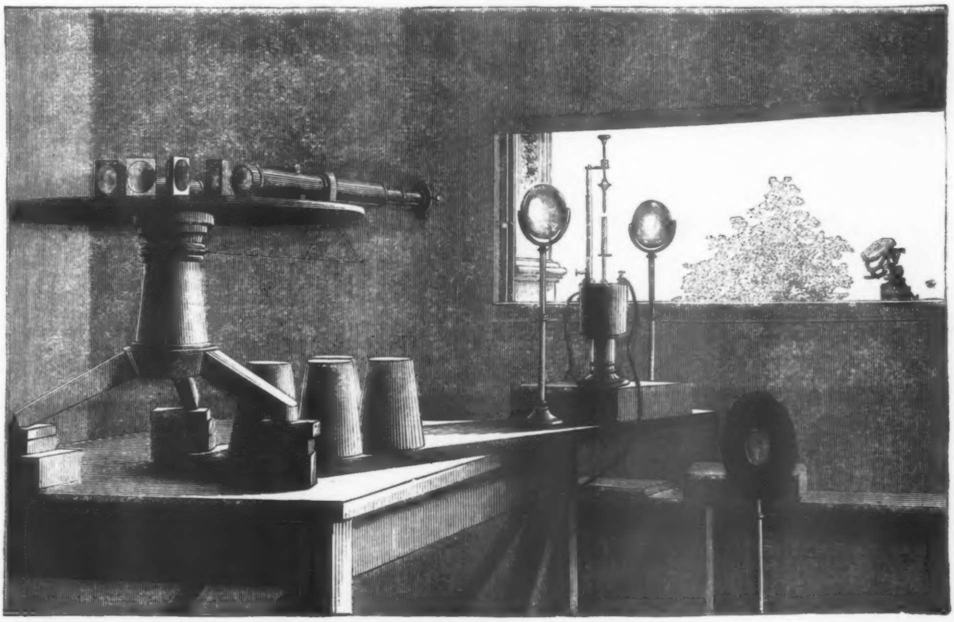

FIG. 31.—Arrangement for photographically determining the coincidence of solar and metallic lines.

a matter of fact three or four thousand photographs have been taken, and a very considerable amount of time (about four years) was consumed in that way.

But it may be said, "Surely if you are going to limit yourself to photography, you will only be dealing with a very small part of the spectrum." My reply to that is that already in the year 1875, when a part of this work had been carried on, other laboratory work had given us reason to believe that what was then being done in photography at the blue end of the spectrum would be done by photography in every other portion, for in fact a spectroscopic study of the behaviour of bodies at low temperature, to which I hope I shall have time to refer, had led several to believe—at all events had led me to believe—that what one got in the text-books about actinism and so on was but a very rough approximation to the truth. We had been taking as the functions of light what were really the functions of the

FINAL REDUCTION—IRON.

Intensity in Sun.	Wave length and length of line.	Coincidences with Short Lines.
1	39 0600 / 2	U 3, Zr 5, Yt 4
3	0622 / 4	Va 4
2	0920 / 3	Va 2, Ba 3
3	1010 / 4	Va 4, Pt 3
2	1648 / 2	Co 3
2	1755 / 3	Mn 3, Ce 4
2	1835 / 4	Os 2
1	2700 / 1	Va 2
1	2950 / 1	Mo 3
3	3023 / 4	Ce 4
5	3435 / 4	U 2
3	3475 / 2	Ba 2, Rh 2
3	3628 / 3	Ta 3
2	3975 / 2	Co 3
3	4026 / 3	Va 5
3	4422 / 4	Mo 3
3	4720 / 2	Yt 5, Th 3
2	5012 / 2	Di 2
2	5160 / 2	Ce 3, Ru 3
2	5210 / 3	W 3
2	5423 / 4	U 3, Mo 3, W 4
3	6215 / 3	Yt 5, Ce 3, Di 2
2	6571 / 2	Zr 2
3	6662 / 2	Th 1
1	7555 / 3	Os 2, Ta 4, Cr 2
3	7578 / 4	Di 2
2	7685 / 2	Va 4
2	8083 / 2	Ti 1
1	8320 / 1	Cr 3
3	9520 / 3	Ru 3
2	9750 / 2	Mo 3

bodies which received it, and it was therefore quite easy to imagine, and one was justified in hoping that as the work went on we should find, that what one particular kind of substance would do for the blue rays another particular kind of substance would do for the red rays and for the green rays, and so on. Capt. Abney in his lectures will show you that the spectro-

scope was no bad guide in that matter, and, thanks to his valuable researches, we are now able to photograph as well, if not better, at the extreme red end of the spectrum than we did at that time—years ago now—in the blue.

Well, then, four years were consumed in the accumulation of these facts. I do not now intend to call attention to the whole of them, but I will take some instances, directing special attention to what happened with regard to the spectrum of iron. This[1] is the final map produced up to a certain point. We have first the solar spectrum; below this are mapped all the lines of iron observed on one of the photographs which we obtained, including of course all impurities; and then follow the spectra of manganese, cobalt, nickel, chromium, uranium, cerium, and so on through the whole story. When that work had been completed in that manner we had to get rid of the impurities by the process which I have already explained, and at last we got what is called a purified spectrum, in which, along the horizon labelled iron we had only those lines left which we could not by any application of the principle which has been explained be shown to be due to the admixture of any other substance whatever. What then was the total result? The accompanying table (p. 320) will show the sort of corner in which we found ourselves after all this work had been accomplished. It gives the list of the iron lines which, after making every allowance for the existence of impurities, were found to coincide with lines in other substances.

It will be seen, for instance, that the two short lines 390600 and 395423 coincided, the first with short lines in uranium, zirconium, and yttrium, the second with short lines in uranium, molybdenum, and tungsten. Similarly there are two short-line coincidences with zirconium, and no less than six with vanadium, and so on. The total gives the coincidence of the lines of all the elements under the conditions that I have drawn attention to. So that the sum total of this really very laborious inquiry with regard to iron was that in the region between 39 and 40, the region including H and K on that map, where, before the introduction of photography, scarcely any iron lines had been seen, and where only five solar lines I think had been mapped, photography gave us a total of nearly 300 lines in the solar spectrum, and it gave us sixty-two lines of iron.

Of those sixty-two lines of iron only eighteen went straight; by which I mean that the remainder had short-line coincidences with the lines of other substances. So that the idea first thrown out by Kirchhoff, Ångström, and Thalèn of the possibility of the coincidence of lines among the metallic elements was enormously intensified. It will be seen that the thing is absolutely reversed in the case of iron, and it might be the case also in other substances. The fact of a line not being coincident with a line in another substance was the exception, and not the rule. The ratio in the case of iron being as 44 to 18.

It is amusing in the light of recent criticisms to go back to the old observations and to see with what pertinacity for the first two years we stuck to the possibility that the solar line or the iron line we were dealing with was a double line, and then, after we had to give that idea up, as the coincidences became of three, four, five, and sixfold complexity, we came to the conclusion that we were dealing with a common impurity. That of course was a point we could not settle until we had gone through all the chemical elements which were known to us, and it was going through so many substances which took up so much time.

But there was another question which became striking, in this excessively minute anatomy of even a very small portion of the solar spectrum, for I should say that the small range of the spectrum represented here forms a portion of a map which, when completed, will be the sixteenth of a mile long, so that after all we were dealing with an excessively small portion of the total work which had to be done. Having there mapped that small region, where without photography it would have been difficult to see any lines at all, we got in almost twenty cases from one end to the other, instances in which there was absolutely no relationship at all between the brightness of the iron line on our photographs and the darkness of the corresponding solar line.

These were carefully noted as "anomalous reversals," a term we coined in the laboratory at the time, and which we still use, although the word anomalous always suggests a very large amount of ignorance.

In more ways than one, then, this work landed us in rather worse confusion than we were in before. What we had to face was (1) the variation in intensity as we passed from earth to sun, a variation so great that in some cases terrestrial lines were missing in the sun, and in others feeble terrestrial lines were greatly interrupted; and (2) the coincidence of lines in several spectra. That is, here and there along the spectrum we found the lines massed as it were even if the coincidence was but apparent, and it really did

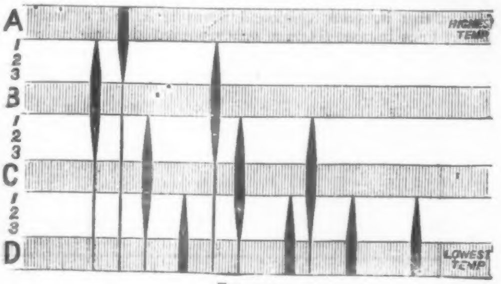

FIG. 32.[1]

seem time to consider what the effect would be, supposing that a dissociation was really going on under our eyes without our knowing or imagining anything about it. Why, it may be said, did you pitch on dissociation? For the reason that the startling results really after all contained nothing that was new—nothing that was novel about them the least in the world, if we regarded them with an absolutely unbiased and receptive mind. Dissociation would undoubtedly account for all the variations of intensity observed on passing from one temperature to another, as already exemplified in the case of the calcium lines, and moreover the short common lines, should they turn out to be truly common, which we were getting in the case of all substances, might be simply the equivalents of those short common lines of calcium which for years past we had watched coming out of the salts of calcium when decomposition was taking place. No new theory was necessary. The appeal to the law of continuity, as I said before, was really open to us, and it seemed to be our duty to appeal to it, and it was also easy to see, before really one has inquired into the matter, that if nature had built up the inorganic world in the way we now know she has built up the organic world, that precisely these facts and none other would be those she would present to us.

"Let us assume a series of furnaces A–D, of which A is the hottest (Fig. 32).

"Let us further assume that in A there exists a substance α, by itself competent to form a componnd body β by union with itself, or with something else when the temperature is lowered.

"Then we may imagine a furnace B in which this compound body exists alone. The spectrum of the compound β would be

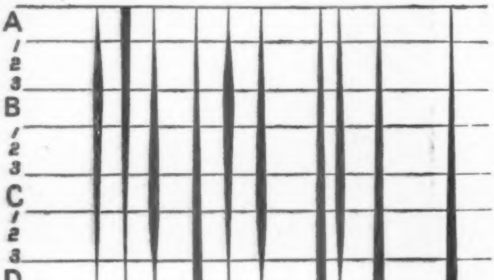

FIG. 33.

the only one visible in B, as the spectrum of the assumed elementary body α would be the only one visible in A.

"A lower temperature furnace C will provide us with a more compound substance γ, and the same considerations will hold good.

[1] This map is too large and detailed to reproduce here.

[1] The figures between the hypothetical spectra point to the gradual change in the interstices of the lines as the spectrum is observed near the temperature of each of the furnaces.

"Now if into the furnace A we throw some of this doubly compounded body γ, we shall get at first an integration of the three spectra to which I have drawn attention; the lines of γ will first be thickest, then those of β; finally α will exist alone, and the spectrum will be reduced to one of the utmost simplicity.

"This is not the only conclusion to be drawn from these considerations. Although we have by hypothesis β, γ, and δ all higher, that is, more compound forms of α, and although the strong lines in the diagram may represent the true spectra of these substances in the furnaces B, C, and D, respectively, yet, in consequence of incomplete dissociation, the strong lines of β will be seen in furnace C, and the strong lines of γ will be seen in furnace D, *all as thin lines*. Thus, although in C we have no line which is not represented in D, the intensities of the lines in C and D are entirely changed.

"The same reasoning therefore which shows how variation in intensity can most naturally explain the short line coincidences—lines which I have termed basic, for the line of α strong in A is *basic* in B, C, and D, the lines of β strong in B are *basic* in C and D, and so on.

"I have prepared another diagram which represents the facts on the supposition that the furnace A, instead of having a temperature sufficient to dissociate β, γ, and δ into α, is far below that stage, although higher than B.

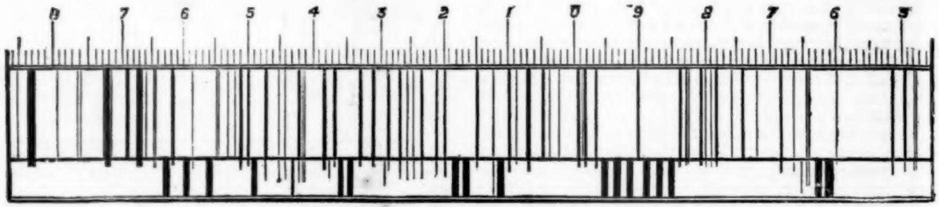

FIG. 34.—Spectrum of sun-spot observed at Greenwich.

"It will be seen from this diagram (Fig. 33) that then the only difference in the spectra of the bodies existing in the four furnaces would consist in the relative thicknesses of the lines. The spectrum of the substances as they exist in A would contain as many lines as would the spectrum of the substances as they exist in D; *each line would in turn be basic in the whole series of furnaces* instead of in one or two only."

We are therefore completely justified in asking whether these are not the differences in intensities of lines to which Kirchhoff and Ångström have referred, and it is quite easy to see that if we change the temperature of the furnaces in such a manner as to produce the strongest lines, owing to the greatest quantity of the vapour given off at any temperature, that the long lines produced at these different temperatures would vary, and the longest line produced in furnace D would not be the same therefore as the longest line produced in furnace A, so that in that way we can imagine a transcendental temperature giving a very long line to a particular substance, and that substance may exist highly compounded in another substance, and yet at a lower temperature it may only appear as an exceedingly short feeble line. The result of this reasoning was, in short, to explain at once variations of intensity of the short feeble lines which were common to so many of the so-called elementary bodies.

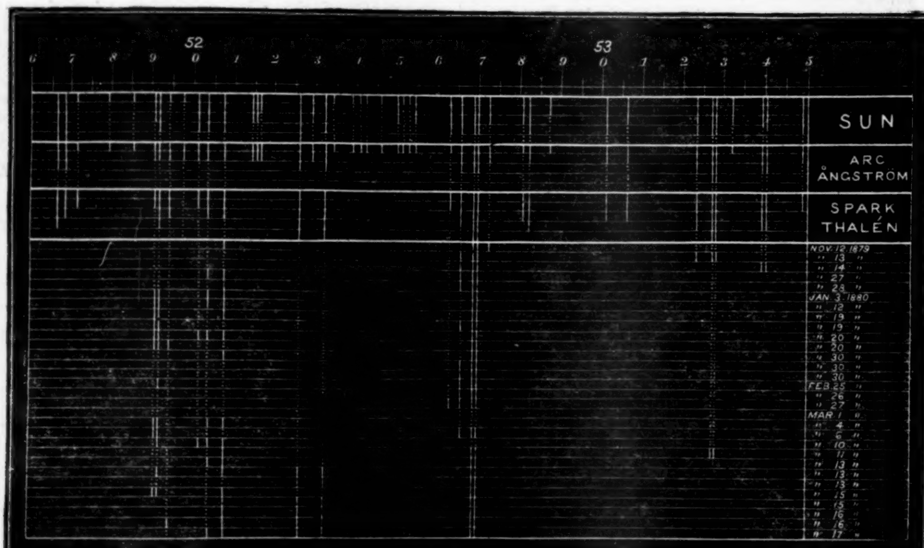

FIG. 35.—Portion of a large map showing the lines most affected in 100 sun-spots observed at South Kensington.

I am particularly anxious to point out that there is absolutely nothing new in these views. We have simply taken as our exemplar the behaviour of a known compound body, and then pushed the reasoning three or four stages further. We have gone just the safest possible way, by the easiest possible stages, from the known to the unknown.

I have now to refer, one by one, to the various tests which have been applied to these considerations, and I should now like to bring the first considerable test under notice. I shall show on a subsequent occasion the various laboratory methods that we possess of determining whether short lines are really the product of high temperature. I shall at once draw your attention to the fact that the short lines may be due, not merely to the work of high temperature, being thus truly produced by the temperature which we are employing, but they may be also the

indications of excessively complex groupings which are just dying at the temperature we are using at the time. So that if it may be permitted to coin terms I should like to call some of the short lines hot-short lines, and others cold-short lines. We shall see the reason by and by.

Now if this order of things is in any way as I have stated it, the first test that we have to employ is one of excessive simplicity. The differences between terrestrial and solar spectra indicate that if the view be correct differences should be seen in the spectra of the same substances observed in different parts of the sun.

We should now have a very distinct notion of the enormous difference of temperature between the highest and lowest reaches of the solar atmosphere. The lowest region of the solar atmosphere that we can get at must be far hotter than the highest part we can get at, at all events in times of eclipses; the lines that we should see therefore in the hottest region of the sun should bring us very near to the effects of this transcendental

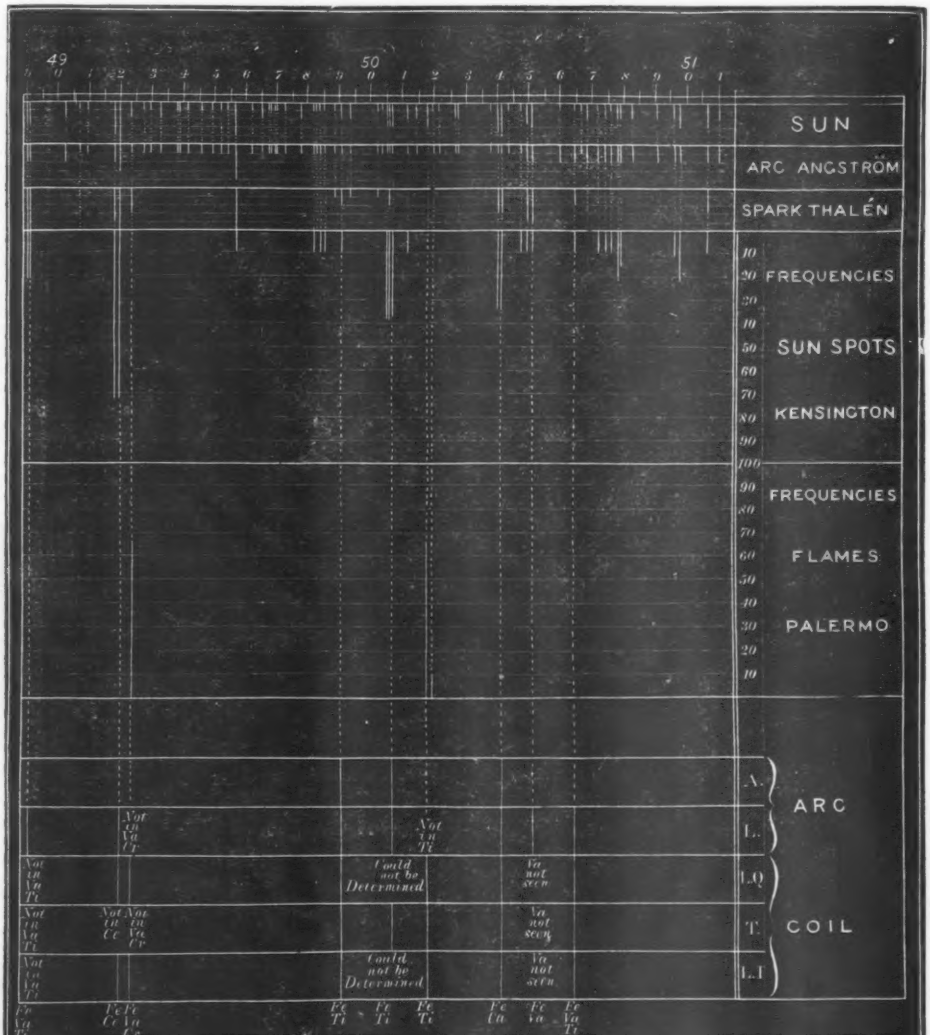

Fig. 36.

temperature to which I have referred, and the spectrum of iron seen in this way should bring us in presence of the result of the highest temperature.

Let us take then the storms as giving us the spectrum of the hottest part of the sun. Where are we to find the record of the coolest part? Now to get to this point we have had naturally to dismiss all the observations which have been made of the lines visible in solar prominences, of the lines thickened in solar spots and the like, because we know that in these prominences and spots we really are dealing with phenomena local to particular and highly heated regions.

Dealing with the whole solar spectrum we know that we are dealing with the whole of the solar atmosphere, however great, however high that atmosphere must be. Therefore we know that the solar atmospheric spectrum, the Fraunhofer spectrum, cannot by any possibility give us what is going on in any particular region—it must naturally be the summation of what is going on in every region where any absorption of any kind

whatever is visible. Therefore as the spectra of prominences and of storms may be stated to be the spectra of the hottest regions of the sun that we can get at in our inquiries. The lines in the solar spectrum affected neither in spots nor flames give us an approach to the cool spectrum we are in search of. We might expect if differences were observable that we should get something like this—

Lines special to prominences ... ... = hottest.
Lines special to spots... ... ... = medium.
Lines affected neither in spots nor storms.. = coolest.

How have these views been tested. The first attempt made to get light out of this inquiry was one which simply dealt with a long catalogue of lines observed by Prof. Young in the memorable expedition of his to Mount Sherman, where, at the height of between 8000 and 9000 feet, with perfect weather and admirable instrumental appliances, about a month was employed in getting such a catalogue of lines as had never been got before. But it was found that, although the result of this inquiry was absolutely in harmony with these views, still after all one wanted more facts. Therefore we have endeavoured to get some of the facts here. And the way in which they have been collected is as follows:—During the last two years the spectra of 100 sun-spots have been observed in the observatory here—observed in a new fashion, and for a good reason I think. In this changeable climate it does not do to do as we began by doing—to attempt to observe all the lines acted upon in a solar spot. The excessive complication, and the intense variation of a spot-spectrum from the ordinary solar spectrum, cannot be better shown than by throwing on the screen the spectrum of one of the sun-spots lately observed at Greenwich.

The figure (Fig. 34) shows a limited part of the solar spectrum, and the lines thickened in the spot-spectrum. It will be seen therefore that to tabulate the existence and thickness and intensities of these lines over the whole of the solar spectrum would be a work which it would be difficult to accomplish in a single day, even if the day were absolutely fine. So that was given up in favour of a limited inquiry over a small part of the solar spectrum; limited further by this, that we only get the twelve lines most affected in each spot on each day. In this way we insure a considerable number of absolutely comparable observations, and we can more easily compare the spot results with those which had been obtained in the observation of the brightest lines in prominences, because when we begin to observe lines in the solar prominences one naturally begins by observing the brightest lines first. So that by observing the darkest lines first in the case of spots, one has a fairer comparison.

A diagram (Fig. 35) will show the result of our observations of 100 spots over a very limited part of the solar spectrum. We will begin by the individual observations. We have at the top the iron lines recorded among the Fraunhofer lines; below we have the iron lines recorded as iron lines by Ångström, who used an electric arc. Lower down we have the iron lines recorded by Thalèn, who used the electric spark. It will be seen that there is a very considerable difference in the spectrum of iron as viewed by means of the spark and by means of the arc, and that there is an equal difference between the spectrum of iron in the sun, that is to say, in the whole sun, determined by the Fraunhofer lines, and the spectrum of either the arc or the spark. It is also to be noted that the solar spectrum is more like the spectrum of the arc than the spectrum of the spark.

Since the relative intensities in all these cases are represented by the length of the lines, we have here an opportunity of observing and discussing the accuracy of Kirchhoff's statement that the iron lines in the sun correspond absolutely in intensity with the lines of iron seen in a light source here. It is necessary first of all to see which light source he fixes on, whether the arc or the spark. When this has been done it is found that the statement is really true with regard to neither.

That however is a digression; to proceed with the diagram, descending from this general spectrum of iron which we get by the absorption of the whole atmosphere of the sun independently of the hottest region and the coldest region—descending from the general to the particular—and taking that particular part of the solar atmosphere where the spots produce their phenomena, let us see what are the results in the case of the spots? We have in the vertical lines a record of the lines which are affected in each spot, and each of the spaces included between the horizontal lines represents a particular spot, the date being given on the right hand side; and these 100 lines which we have here represent the phenomena produced by 100 spots. The diagram is a small portion of the larger map. Now the wonderful thing that one is at once struck with is the absolute and complete irregularity of the whole result. There is no continuity among any of these lines. A careful inspection of the diagram shows us that, speaking in a general way, each of these lines is seen in one spot or another absolutely without the other. We have an *inversion* in the intensities of the lines when passing from spot to spot. Whenever we get a line intensified by Thalèn, we miss it in the spots, and, as a rule, what happens is that the spectrum of the spot is not only simpler than the spectrum of the arc, but simpler than the spectrum of the spark.

Now the importance of these statements depends on other statements which we can bring to confront with them. The next diagram shows the observations of 100 prominences observed between the years 1872 and 1876. (The diagram was thrown on the screen.) Prominences exist in a region of the solar atmosphere not very far from that occupied by the spots, but we have already seen that whereas the spots are produced by a downrush of cool material, prominences are produced by an uprush of hot material. Let us see therefore if any change is produced in the phenomena; whether we shall have exactly the same lines from the flames, or the prominences, as we have from the spots; whether we shall get the same information or no.

Here are the facts with respect to Tacchini's observations:—We begin as before with the whole absorption of the sun, Ångström's map, and Thalèn's map. I think you will see a very considerable change; the iron lines (for we are only dealing with iron) most prominent in the prominences are vastly different from the iron lines most thickened in the spots. The difference is shown in the annexed diagram (Fig. 36), which represents those individual observations both of spots and flames treated in a certain way with reference to the discussion. I will at once explain to you what that certain way is. We have, as before, the three data to begin with, and we have treated the sun-spot observations so that the lengths of the lines will represent the number of times they have been seen in 100 sun-spots; the line at wave-length 4919·5, for instance, has been seen seventy-two times; *that* line, in fact, has been seen more than any other; the one at 5005·0 some forty times, and so on; very many lines having been seen less than ten times. In another part of the same diagram we have summarised the individual results obtained from Tacchini's observation of prominences in exactly the same way. The line 5017·5 was seen in 66 prominences out of 100. But why I am particularly anxious to show this diagram is this, that it brings out the perfectly natural fact—for it is the natural fact—that over this region of the spectrum, at all events, no iron lines affected in the spots are visible in the prominences. If we assume that the region occupied by prominences is hotter than the region occupied by spots, that higher region ought to do this work, and it ought to be a work of simplification. Therefore I say it is a perfectly natural result, and not one to be wondered at, that in the spectra of the flames there is no line coincident with any of the lines seen frequently widened in the spots.

Now we have these three solar spectra here which we can compare one with the other. First of all we have the iron spectrum of the sun taken as a whole. Then we have next the spectrum of spots, which we know to be hotter than the sun taken as a whole. Then we have the spectrum of flames, which we know to be hotter than the spots. It will be seen that the story, as it runs from the top of the diagram downwards, is a story of greater simplicity, as it ought to be, and it was explained in the diagram which I exhibited before I began to show these results of absolute hard facts. It will be seen that the simplicity brought about by the reduction of lines actually seen as to number, is accompanied by the appearance of new lines (produced by the transcendental temperatures) in these regions. This first discussion of a large number of spectra and of spots, as compared with storms, is, I submit, in absolute harmony with the view of the dissociation of the elementary bodies by the solar temperature suggested by Sir Benjamin Brodie in 1867, and therefore I may further add that to me, at all events, it is absolutely inexplicable on any other view.

J. NORMAN LOCKYER

(*To be continued.*)

INTERNATIONAL MEDICAL CONGRESS

THIS Congress, which opened by an informal reception at the College of Physicians on Tuesday, has so far been a real success. It has brought together something

like 2500 medical men, no less than 1000 being from abroad, and 500 from the provinces. Indeed, the attendance is more than double that of any previous Congress. Among the distinguished foreigners who attend the Congress are the following:—Dr. Fordyce Barker, New York; Dr. Billings, Washington; Dr. Bigelow, Boston; Professors Brown-Séquard, Paris; Chauveau, Lyons; Donders, Utrecht; Professors Holmgren, Upsala; His, Leipsic; Kölliker, Wurzburg; Klebs, Prague; Loven, Stockholm; Pasteur, Paris; Pflüger, Bonn; Panteleoni, Rome; Von Slawjansky, St. Petersburg; Stokvis, Amsterdam; Virchow, Berlin. A very large concourse of members thronged the rooms of the College on Tuesday, and crowded St. James's Hall yesterday morning, when Sir James Paget delivered the presidential address. The sectional meetings are being held in the rooms of the various scientific societies in the Burlington House region, and there are fifteen of them altogether. Prof. Virchow gave an address last night on "The Value of Pathological Experiments." To-day Prof. Maurice Raynaud gives a general address on "Scepticism in Medicine"; to-morrow Dr. Billings of Washington gives an address on "Our Medical Literature"; and to-morrow night the Lord Mayor and Corporation receive the members in the Guildhall at a *conversazione*. On Saturday there will be several excursions, and Sir Joseph Hooker will hold a reception at Kew in the afternoon. On Monday at a general meeting Prof. Volkmann of Halle will lecture on "Modern Surgery"; and on Tuesday Prof. Huxley will lecture on "The Connection of the Biological Sciences with Medicine." We this week give the opening address of Sir James Paget:—

As I look round this hall my admiration is moved not only by the number and total power of the minds which are here, but by their diversity, a diversity in which I believe they fairly represent the whole of those who are engaged in the cultivation of our science. For here are minds representing the distinctive characters of all the most gifted and most educated nations; characters still distinctly national, in spite of the constantly increasing intercourse of the nations. And from many of these nations we have both elder and younger men; thoughtful men and practical; men of fact and men of imagination; some confident, some sceptic; various, also, in education, in purpose and mode of study, in disposition, and in power. And scarcely less various are the places and all the circumstances in which those who are here have collected and have been using their knowledge. For I think that our calling is pre-eminent in its range of opportunities for scientific study. It is not only that the pure science of human life may match with the largest of the natural sciences in the complexity of its subject-matter; not only that the living human body is, in both its material and its indwelling forces, the most complex thing yet known, but that in our practical duties this most complex thing is presented to us in an almost infinite multiformity. For in practice we are occupied, not with a type and pattern of the human nature, but with all its varieties in all classes of men, of every age and every occupation, and all climates and all social states; we have to study men singly and in multitudes, in poverty and in wealth, in wise and unwise living, in health and all the varieties of disease; and we have to learn, or at least try to learn, the results of all these conditions of life while, in successive generations and in the mingling of families, they are heaped together, confused, and always changing. In every one of all these conditions man, in mind and body, must be studied by us; and every one of them offers some different problems for inquiry and solution. Wherever our duty or our scientific curiosity, or, in happy combination, both, may lead us, there are the materials and there the opportunities for separate original research.

Now, from these various opportunities of study, men are here in Congress. Surely, whatever a multitude and diversity of minds can in a few days do for the promotion of knowledge, may be done here.

But it is not proposed to leave the work of the Congress to what would seem like chances and disorder, good as the result might be; nor yet to the personal influences by which we may all be made fitter for work, though these may be very potent. In the stir and controversy of meetings such as we shall have, there cannot fail to be useful emulation; by the examples that will appear of success in research, many will be moved to more enthusiasm, many to more keen study of the truth; our range of work will be made wider, and we shall gain that greater interest in each other's views and that clearer apprehension of them which are always attained by personal acquaintance and by memories of association in pleasure as well as in work. But as it will not be left to chance, so neither will sentiment have to fulfil the chief duties of the Congress.

Following the good example of our predecessors, certain subjects have been selected which will be chiefly, though not exclusively, discussed, and the discussions are to be in the sections into which we shall soon divide.

Of these subjects it would not be for me to speak even if I were competent to do so; unless I may say that they are so numerous and complete that—together with the opening addresses of the Presidents of Sections—they leave me nothing but such generalities as may seem commonplace. They have been selected, after the custom of former meetings, from the most stirring and practical questions of the day; they are those which must occupy men's minds, and on which there is at this time most reason to expect progress, or even a just decision, from very wide discussion. They will be discussed by those most learned in them, and in many instances by those who have spent months or years in studying them, and who now offer their work for criticism and judgment.

I will only observe that the subjects selected in every section involve questions in the solution of which all the varieties of mind and knowledge of which I have spoken may find their use. For there are questions, not only on many subjects, but in all stages of progress towards settlement. In some the chief need seems to be the collection of facts well observed by many persons. I say by many, not only because many facts are wanted, but because in all difficult research it is well that each apparent fact should be observed by many; for things are not what they appear to each one mind. In that which each man believes that he observes, there is something of himself; and for certainty, even on matters of fact, we often need the agreement of many minds, that the personal element of each may be counteracted. And much more is this necessary in the consideration of the many questions which are to be decided by discussing the several values of admitted facts and of probabilities, and of the conclusions drawn from them. For, on questions such as these minds of all kinds may be well employed. Here there will be occasion even for those which are not unconditionally praiseworthy, such as those that habitually doubt, and those to whom the invention of arguments is more pleasing than the mere search for truth. Nay, we may be able to observe the utility even of error. We may not indeed wish for a prevalence of errors; they are not more desirable than are the crime and misery which evoke charity. And yet in a congress we may palliate them, for we may see how, as we may often read in history, errors, like doubts and contrary pleadings, serve to bring out the truth, to make it express itself in clearest terms and show its whole strength and value. Adversity is an excellent school for truth as well as for virtue.

But that which I would chiefly note, in relation to the great variety of minds which are here, is that it is characteristic of that mental pliancy and readiness for variation which is essential to all scientific progress, and which a great international congress may illustrate and promote. In all the subjects for discussion we look for the attainment of some novelty and change in knowledge or belief; and after every such change there must ensue a change in some of the conditions of thinking and of working. Now, for all these changes minds need to be pliant and quick to adjust themselves. For all progressive science there must be minds that are young whatever may be their age.

Just as the discovery of auscultation brought to us the necessity for a refined cultivation of the sense of hearing, which was before of only the same use in medicine as in the common business of life; or, as the employment of the numerical method in estimating the value of facts required that minds should be able to record and think in ways previously unused; or, as the acceptance of the doctrine of evolution has changed the course of thinking in whole departments of science: so is it, in less measure, in every less advance of knowledge. All such advances change the circumstances of the mental life, and minds that cannot or will not adjust themselves become less useful, or must at least modify their manner of utility. They may continue to

be the best defenders of what is true; they may strengthen and expand the truth, and may apply it in practice with all the advantages of experience; they may thus secure the possessions of science and use them well; but they will not increase them.

It is with minds as with living bodies. One of their chief powers is in their self-adjustment to the varying conditions in which they have to live. Generally those species are the strongest and most abiding that can thrive in the widest range of climate and of food. And of all the races of men they are the mightiest and most noble who are, or by self-adjustment can become, most fit for all the new conditions of existence in which by various changes they may be placed. These are they who prosper in great changes of their social state; who, in successive generations, grow stronger by the production of a population so various that some are fitted to each of all the conditions of material and mode of life which they can discover or invent. These are most prosperous in the highest civilisation; these whom nature adapts to the products of their own arts.

Or, among other groups, the mightiest are those who are strong alike on land and sea; who can explore and colonise, and in every climate can replenish the earth and subdue it; and this not by tenacity or mere robustness, but rather by pliancy and the production of varieties fit to abide and increase in all the various conditions of the world around.

Now it is by no distant analogy that we trace the likeness between these in their successful contests with the material conditions of life and those who are to succeed in the intellectual strife with the difficulties of science and of art. There must be minds which in variety may match with all the varieties of the subject-matters and minds which, at once or in swift succession, can be adjusted to all the increasing and changing modes of thought and work.

Such are the minds we need; or rather, such are the minds we have; and these in great meetings prove and augment their worth. Happily the natural increase in the variety of minds in all cultivated races is—whether as cause or as consequence—nearly proportionate to the increasing variety of knowledge. And it has become proverbial, and is nearly true in science and art, as it is in commerce and in national life, that, whatever work is to be done, men are found or soon produced who are exactly fit to do it.

But it need not be denied that, in the possession of this first and chiefest power for the increase of knowledge, there is a source of weakness. In works done by dissimilar and independent minds, dispersed in different fields of study, or only gathered into self-assorted groups, there is apt to be discord and great waste of power. There is therefore need that the workers should from time to time be brought to some consent and unity of purpose; that they should have opportunity for conference and mutual criticism, for mutual help and the tests of free discussion. This it is which, on the largest scale and most effectually, our Congress may achieve; not indeed by striving after a useless and happily impossible uniformity of mind or method, but by diminishing the lesser evil of waste and discord which is attached to the far greater good of diversity and independence. Now as in numbers and variety the Congress may represent the whole multitude of workers everywhere dispersed, so in its gathering and concord it may represent a common consent that, though we may be far apart and different, yet our work is and shall be essentially one; in all its parts mutually dependent, mutually helpful, in no part complete or self-sufficient. We may thus declare that as we who are many are met to be members of one body, so our work for science shall be one, though manifold; that as we, who are of many nations, will for a time forget our nationalities and will even repress our patriotism, unless for the promotion of a friendly rivalry, so will we in our work, whether here and now or everywhere and always, have one end and one design—the promotion of the whole science and whole art of healing.

It may seem to be a denial of this declaration of unity that, after this general meeting, we shall separate into sections more numerous than in any former Congress. Let me speak of these sections to defend them; for some maintain that, even in such a division of studies as these may encourage, there is a mischievous dispersion of forces. The science of medicine, which used to be praised as one and indivisible, is broken-up, they say, among specialists, who work in conflict rather than in concert, and with mutual distrust more than mutual help.

But let it be observed that the sections which we have instituted are only some of those which are already recognised in many countries, in separate societies, each of which has its own place and rules of self-government and its own literature. And the division has taken place naturally in the course of events which could not be hindered. For the partial separation of medicine, first from the other natural sciences, and now into sections of its own, has been due to the increase of knowledge being far greater than the increase of individual mental power.

I do not doubt that the average mental power constantly increases in the successive generations of all well-trained peoples; but it does not increase so fast as knowledge does, and thus in every science, as well as in our own, a small portion of the whole sum of knowledge has become as much as even a large mind can hold and duly cultivate. Many of us must, for practical life, have a fair acquaintance with many parts of our science, but none can hold it all; and for complete knowledge, or for research, or for safely thinking-out beyond what is known, no one can hope for success unless by limiting himself within the few divisions of the science for which, by nature or by education, he is best fitted. Thus, our division into sections is only an instance of that division of labour which, in every prosperous nation, we see in every field of active life and which is always justified by more work better done.

Moreover, it cannot be said that in any of our sections there is not enough for a full strong mind to do. If any one will doubt this let him try his own strength in the discussions of several of them.

In truth, the fault of specialism is not in narrowness, but in the shallowness and the belief in self-sufficiency with which it is apt to be associated. If the field of any specialty in science be narrow, it can be dug deeply. In science, as in mining, a very narrow shaft, if only it be carried deep enough, may reach the richest stores of wealth and find use for all the appliances of scientific art. Not in medicine alone, but in every department of knowledge, some of the grandest results of research and of learning, broad and deep, are to be found in monographs on subjects that, to the common mind, seemed small and trivial.

And study in a Congress such as this may be a useful remedy for self-sufficiency. Here every group may find a rare occasion, not only for an opportune assertion of the supreme excellence of its own range and mode of study, but for the observation of the work of every other. Each section may show that its own facts must be deemed sure, and that by them every suggestion from without must be tested; but each may learn to doubt every inference of its own which is not consistent with the facts or reasonable beliefs of others; each may observe how much there is in the knowledge of others which should be mingled with its own; and the sum of all may be the wholesome conviction of all, that we cannot justly estimate the value of a doctrine in one part of our science till it has been tried in many or in all.

We were taught this in our schools; and many of us have taught that all the parts of medical science are necessary to the education of the complete practitioner. In the independence of later life some of us seem too ready to believe that the parts we severally choose may be self-sufficient, and that what others are learning cannot much concern us. A fair study of the whole work of the Congress may convince us of the fallacy of this belief. We may see that the test of truth in every part must be in the patient and impartial trial of its adjustment with what is true in every other. All perfect organisations bear this test; all parts of the whole body of scientific truth should be tried by it.

Moreover, I would not, from a scientific point of view, admit any estimate of the comparative importance of the several divisions of our science, however widely they may differ in their present utilities. And this I would think right, not only because my office as president binds me to a strict impartiality and to the claim of freedom of research for all, but because we are very imperfect judges of the whole value of any knowledge, or even of single facts. For every fact in science, wherever gathered, has not only a present value, which we may be able to estimate, but a living and germinal power of which none can guess the issue.

It would be difficult to think of anything that seemed less likely to acquire practical utility than those researches of the few naturalists who, from Leeuwenhoeck to Ehrenberg, studied the most minute of living things, the Vibrionidæ. Men boasting themselves as practical might ask, "What good can come of it?" Time and scientific industry have answered, "This good: those researches have given a more true form to one of the most important practical doctrines of organic chemistry; they have introduced a great beneficial change in the most practical part of

surgery; they are leading to one as great in the practice of medicine; they concern the highest interests of agriculture, and their power is not yet exhausted.

And as practical men were, in this instance, incompetent judges of the value of scientific facts, so were men of science at fault when they missed the discovery of anæsthetics. Year after year the influences of laughing-gas and of ether were shown: the one fell to the level of the wonders displayed by itinerant lecturers, students made fun with the other; they were the merest practical men, men looking for nothing but what might be straightway useful, who made the great discovery which has borne fruit not only in the mitigation of suffering, but in a wide range of physiological science.

The history of science has many similar facts, and they may teach that any man will be both wise and dutiful if he will patiently and thoughtfully do the best he can in the field of work in which, whether by choice or chance, his lot is cast. There let him, at least, search for truth, reflect on it, and record it accurately; let him imitate that accuracy and completeness of which I think we may boast that we have, in the descriptions of the human body, the highest instance yet attained in any branch of knowledge. Truth so recorded cannot remain barren.

In thus speaking of the value of careful observation and records of facts, I seem to be in agreement with the officers of all the sections; for, without any intended consent, they have all proposed such subjects for discussion as can be decided only by well-directed facts and fair direct inductions from them. There are no questions on theories or mere doctrines. This, I am sure, may be ascribed, not to any disregard of the value of good reasoning or of reasonable hypotheses, but partly to the just belief that such things are ill-suited for discussion in large meetings, and partly to the fact that we have no great opponent schools, no great parties named after leaders or leading doctrines about which we are in the habit of disputing. In every section the discussions are to be on definite questions, which, even if they be associated with theory or general doctrines, may yet be soon brought to the test of fact; there is to be no use of doctrinal touchstones.

I am speaking of no science but our own. I do not doubt that in others there is advantage in dogma, or in the guidance of a central organising power, or in divisions and conflicting parties. But in the medical sciences I believe that the existence of parties founded on dominant theories has always been injurious; a sign of satisfaction with plausible errors, or with knowledge which was even for the time imperfect. Such parties used to exist, and the personal histories of their leaders are some of the most attractive parts of the history of medicine: but, although in some instances an enthusiasm for the master-mind may have stirred a few men to unusual industry, yet very soon the disciples seem to have been fascinated by the distinctive doctrine, content to bear its name, and to cease from active scientific work. The dominance of doctrine has promoted the habit of inference, and repressed that of careful observation and induction. It has encouraged that fallacy to which we are all too prone, that we have at length reached an elevated sure position on which we may rest, and only think and guide. In this way specialism in doctrine or in method of study has hindered the progress of science more than the specialism which has attached itself to the study of one organ or of one method of practice. This kind of specialism may enslave inferior minds: the specialism of doctrine can enchant into mere dreaming those that should be strong and alert in the work of free research.

I speak the more earnestly of this because it may be said, if our Congress be representative, as it surely is, may we not legislate? May we not declare some general doctrines which may be used as tests and as guides for future study? We had better not.

The best work of our International Congress is in the clearing and strengthening of the knowledge of realities; in bringing, year after year, all its force of numbers and varieties of minds to press forward the demonstration and diffusion of truth as nearly to completion as may from year to year be possible. Thus, chiefly, our Congress may maintain and invigorate the life of our science. And the progress of science must be as that of life. It sounds well to speak of the temple of science, and of building and crowning the edifice. But the body of science is not as any dead thing of human work, however beautiful; it is as something living, capable of development and a better growth in every part. For, as in all life the attainment of the highest condition is only possible through the timely passing-by of the less good, that it may be replaced by the better, so is it in science. As time passes, that which seemed true and was very good becomes relatively imperfect truth, and the truth more nearly perfect takes its place.

We may read the history of the progress of truth in science as a paleontology. Many things which, as we look far back, appear, like errors, monstrous and uncouth creatures, were, in their time, good and useful, as good as possible. They were the lower and less perfect forms of truth which, amid the floods and stifling atmospheres of error, still survived; and just as each successive condition of the organic world was necessary to the evolution of the next following higher state, so from these were slowly evolved the better forms of truth which we now hold.

This thought of the likeness between the progress of scientific truth and the history of organic life may give us all the better courage in a work which we cannot hope to complete, and in which we see continual, and sometimes disheartening, change. It is, at least, full of comfort to those of us who are growing old. We that can read in memory the history of half a century might look back with shame and deep regret at the imperfections of our early knowledge if we might not be sure that we held, and sometimes helped onward, the best things that were, in their time, possible, and that they were necessary steps to the better present, even as the present is to the still better future. Yes—to the far better future; for there is no course of nature more certain than is the upward progress of science. We may seem to move in circles, but they are the circles of a constantly ascending spiral; we may seem to sway from side to side, but it is only as on a steep ascent which must be climbed in zig-zag.

What may be the knowledge of the future none can guess. If we could conceive a limit to the total sum of mental power which will be possessed by future multitudes of well-instructed men, yet could we not conceive a limit to the discovery of the properties of materials which they will bend to their service. We may find the limit of the power of our unaided limbs and senses; but we cannot guess at a limit to the means by which they may be assisted, or to the invention of instruments which will become only a little more separate from our mental selves than are the outer sense-organs with which we are constructed.

In the certainty of this progress the great question for us is what shall we contribute to it? It will not be easy to match the recent past. The advance of medical knowledge within one's memory is amazing, whether reckoned in the wonders of the science not yet applied, or in practical results in the general lengthening of life, or, which is still better, in the prevention and decrease of pain and misery, and in the increase of working power. I cannot count or recount all that in this time has been done; and I suppose there are very few, if any, who can justly tell whether the progress of medicine has been equal to that of any other great branch of knowledge during the same time. I believe it has been; I know that the same rate of progress cannot be maintained without the constant and wise work of thousands of good intellects; and the mere maintenance of the same rate is not enough, for the rate of the progress of science should constantly increase. That in the last fifty years was at least twice as great as that in the previous fifty. What will it be in the next, or, for a more useful question, what shall we contribute to it?

I have no right to prescribe for more than this week. In this let us do heartily the proper work of the Congress, teaching, learning, discussing, looking for new lines for research, planning for mutual help, forming new friendships. It will be hard work if we will do it well; but we have not met for mere amusement or for recreation, though for that I hope you will find fair provision, and enjoy it the better for the work preceding it.

And when we part let us bear away with us, not only much more knowledge than we came with, but some of the lessons for our conduct in the future which we may learn in reflecting the work of our Congress.

In the number and intensity of the questions brought before us, we may see something of our responsibility. If we could gather into thought the amounts of misery or happiness, of helplessness or of power for work, which may depend on the answers to all the questions that will come before us, this might be a measure of our responsibility. But we cannot count it; let us imagine it; we cannot even in imagination exaggerate it. Let us bear it always in our mind, and remind ourselves that our responsibility will constantly increase. For, as men become in the best sense better educated, and the influence of scientific knowledge on their moral and social state

increases, so among all sciences there is none of which the influence, and therefore the responsibility, will increase more than ours, because none more intimately concerns man's happiness and working power.

But, more clearly in the recollections of the Congress, we may be reminded that in our science there may be, or, rather, there really is, a complete community of interest among men of all nations. On all the questions before us we can differ, discuss, dispute, and stand in earnest rivalry; but all consistently with friendship, all with readiness to wait patiently till more knowledge shall decide which is in the right. Let us resolutely hold to this when we are apart: let our internationality be a clear abiding sentiment, to be, as now, declared and celebrated at appointed times, but never to be forgotten; we may, perhaps, help to gain a new honour for science, if we thus suggest that in many more things, if they were as deeply and dispassionately studied, there might be found the same complete identity of international interests as in ours.

And then, let us always remind ourselves of the nobility of our calling. I dare to claim for it, that among all the sciences, ours, in the pursuit and use of truth, offers the most complete and constant union of those three qualities which have the greatest charm for pure and active minds—novelty, utility, and charity. These three, which are sometimes in so lamentable disunion, as in the attractions of novelty without either utility or charity, are in our researches so combined that, unless by force or wilful wrong, they hardly can be put asunder. And each of them is admirable in its kind. For in every search for truth we can not only exercise curiosity, and have the delight—the really elemental happiness—of watching the unveiling of a mystery, but, on the way to truth, if we look well round us, we shall see that we are passing wonders more than the eye or mind can fully apprehend. And as one of the perfections of nature is that in all her works wonder is harmonised with utility, so is it with our science. In every truth attained there is utility either at hand or among the certainties of the future. And this utility is not selfish: it is not in any degree correlative with money-making; it may generally be estimated in the welfare of others better than in our own. Some of us may indeed make money and grow rich; but many of those that minister even to the follies and vices of mankind can make much more money than we. In all things costly and vainglorious they would far surpass us if we would compete with them. We had better not compete where wealth is the highest evidence of success; we can compete with the world in the nobler ambition of being counted among the learned and the good who strive to make the future better and happier than the past. And to this we shall attain if we will remind ourselves that, as in every pursuit of knowledge there is the charm of novelty, and in every attainment of truth utility, so in every use of it there may be charity. I do not mean only the charity which is in hospitals or in the service of the poor, great as is the privilege of our calling in that we may be its chief ministers; but that wider charity which is practised in a constant sympathy and gentleness, in patience and self-devotion. And it is surely fair to hold that, as in every search for knowledge we may strengthen our intellectual power, so in every practical employment of it we may, if we will, improve our moral nature; we may obey the whole law of Christian love, we may illustrate the highest induction of scientific philanthropy.

Let us, then, resolve to devote ourselves to the promotion of the whole science, art, and charity of medicine. Let this resolve be to us as a vow of brotherhood; and may God held us in our work.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, July 25.—M. Wurtz in the chair.—The following papers were read:—On the comet *b* of 1881, by M. Mouchez. The result of M. Oudemans' search among the Dutch Colonial Archives in South Africa is that the comet of 1881 is probably not that of 1807, but seen now for the first time.—Determination of the horizontal and lateral flexure and the flexure of the instrumental axis of the meridian circle of Bischoffsheim, by means of new apparatus, by MM. Lœwy and Perigaud.—On the equivalence of quadratic forms, by M. Jordan.—On chlorhydric ether of glycol, by M. Berthelot.—Anthracic vaccination; *résumé* of experiments made at Lambert, near Chartres, to test the method of M. Pasteur, by M. Bouley. The essence of the test consisted in inoculating vaccinated sheep with natural virus (anthracic blood from a sheep which died of the disease) instead of that prepared by processes of culture. The efficacy of the vaccination was fully demonstrated.—On the irreducible covariants of the binary quantic of the eighth order, by Prof. Sylvester.—Parabolic elements of the comet *b* 1881, by M. Bigourdan.—Observations of Schæberle's comet (*c* 1881) at Paris Observatory, by M. Bigourdan; also by MM. Henry.—Considerations on the forces of nature; inadmissibility of the hypothesis proposed by M. Faye to explain the tails of comets, by M. Picard. Whatever the nature of the repulsive force it can only be proportional to masses, not to surfaces, for *ideal* pressure on surfaces only arises from *effective* action on masses. No interposed matter can weaken or arrest its action, for the etherised medium penetrates all bodies. The action is propagated, not successively but instantaneously, being due not to an undulatory motion, but to shocks of etherised atoms and ponderable molecules, like gravitation; hence on a point in motion it is exerted in the same direction as the attraction exercised by the ponderable mass of the sun.—Remarks on the calculation of relative perturbations, according to M. Gyldén's method, by M. Callandreau.—Hemihedral crystals with inclined faces as constant sources of electricity, by MM. Jacques and Pierre Curie. A plate suitably cut in such a crystal and placed between two sheets of tin forms a condenser which becomes charged when it is compressed. The authors give an absolute measure of the quantities of electricity liberated by tourmaline and quartz for a determinate pressure. It is shown how the instrument may serve in comparison of charges and capacities.—Determination of the angular distance of colours, by M. Rosenstiehl. He shows that three colours previously referred to, viz. orange, the third yellow-green, and the third blue, have the characters of a triad (that is, mixed in equal intensity, they produce the sensation of white). All the colours which occupy the angles of an inscribed equilateral triangle have the same properties.—Electric stopcock; transformation, transport, and use of energy, by M. Cabanellas.—On the heat of formation of explosives, by MM. Sarrau and Vieille. When an explosive is decomposed the heat liberated is equal (according to thermodynamics) to the excess of the heat of formation of the products over the heat of formation of the explosive. Hence, knowing, in a given case, the heat liberated by decomposition, and the composition of the products of the reaction, the heat of formation may be arrived at. The authors have applied the method to the principal explosives, and will shortly give the results.—Industry of magnesia (continued), by M. Schlœsing. He treats sewage matter with phosphate of magnesia, obtaining the phosphoric acid from natural phosphates of lime, and the magnesia from sea-water or water of salt marshes (it is precipitated by slaked lime). He produces a sort of vermicelli of lime, which gives a porous magnesia, on which the acid liquid acts easily.—On some reactions of morphine and its congeners, by M. Grimaux.—On a new process of vaccination of chicken cholera, by M. Toussaint. He inoculated fowls with blood of rabbits which had died of septicemia (or with matter cultivated from it), and the effects were those of an attenuated virus, which made the fowls refractory to cholera.—On a volcanic breccia capable of being utilised as an agricultural manure, by M. Carnot. The rock (from l'Herault) contains notable amounts of iron, lime, potash, and phosphoric acid.—Boric acid; its existence in salt lakes of the modern period, and in natural saline waters (second note), by M. Dieulafait.—On the extraordinary temperature of July, 1881, by M. Renou. It rose to 38°·4 on the 19th at the Park Observatory, a degree never experienced in Algiers, the Antilles, and Cayenne.

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